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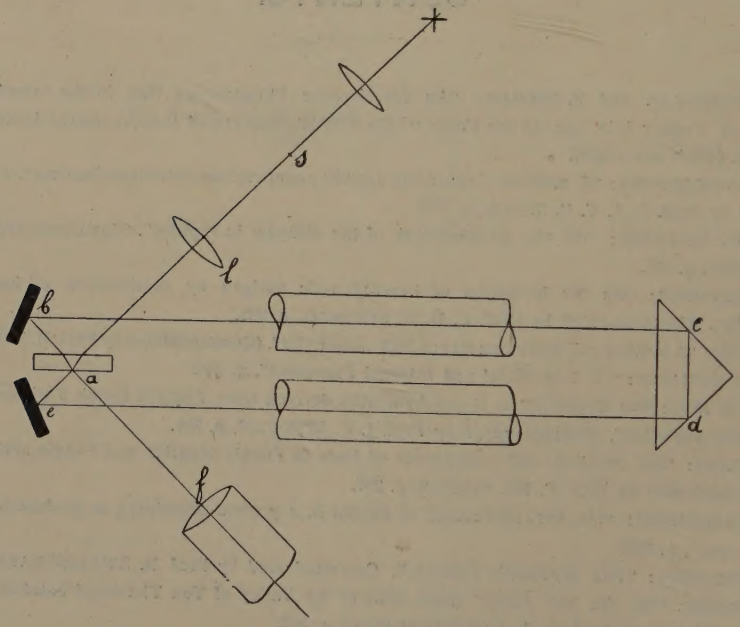
Physics. — "*An Interference Phenomenon Due to the Introduction of Sodium Vapour into one of the Paths of the* FIZEAU-MICHELSON *Interferometer-Arrangement*". By G. C. DIBBETZ Jr. and Prof. P. ZEEMAN.

(Communicated at the meeting of November 26, 1921).

When sodium vapour is introduced into one of the paths of light of the interference-arrangement used in FIZEAU's experiment, surprising shifts of interference fringes may be observed. In order to observe these shifts we must arrange that the sodium vapour acts as a prism, and that by means of a spectroscope the change of the distance of the interference fringes with wavelength can be watched by projecting the interference fringes on the slit of the spectroscope.



The above figure is reproduced from an earlier paper[1]) on FRESNEL's convection coefficient for light of different colours in water, in which use is made of MICHELSON and MORLEY's interferometer. In *a* the ray meets MICHELSON's slightly silvered mirror. There the

[1]) ZEEMAN. These Proc. Vol. XVII p. 445 (June 1914).

incident beam *s l a* is divided into a reflected and a transmitted one. The reflected one follows the path *a b c d e a f*, the transmitted one the path *a e d c b a f*. In some experiments the flame of a MEKÈR-burner, in which a platinum spoon with common salt was placed, was put between *e* and *d*. In another case a greater gradient of density of the sodium vapour was obtained by introducing sodium into an iron tube connected with an air-pump and closed on both sides with glass plates. By heating this tube at the bottom and cooling it at the top, the desired density distribution could be obtained.

With the flame between *e* and *d* the phenomenon was observed reproduced on the Plate in Fig. 1.

Fig. 2 represents the interference phenomenon with a sodium prism of greater density. Both photos were taken with a spectroscope with one glass prism; the first is enlarged 7 times, the second 4 times. In Fig. 1 the two absorption *D*-lines are visible. In fig. 2 the whole region round the *D*-lines has disappeared.

It will not be attempted here to give a full explanation; it would claim more space than we can afford to give it. Even without the sodium vapour the explanation of the dependence of the interference fringes on the mutual position of the five reflecting planes and on the thickness of the slightly silvered mirror is a rather complicated problem, which has not been treated in detail as yet for so far as we know.

We shall confine ourselves to a few remarks on circumstances that play a rôle in the appearance of the phenomenon.

A point to which we draw attention is the particularity that the rays which travel in opposite directions, are deflected by a different amount after their passage through the sodium prism. For if the vapour prism is placed between *e* and *d*, ray *a e d* will have to travel over the long path *d c b a f*, before it reaches the object glass of the telescope, *a b c d* on the other hand only the short path *e a f*.

In this connection we may still mention that nothing has ever appeared of a dependence of the velocity of propagation of light on the intensity. We assume that this does not exist, else this dependence would already give rise to phase differences in our experiment, because one ray is weakened at the beginning of its way, another at the end.

The following auxiliary experiment is also of importance. We introduce a screen with a narrow aperture between *l* and *a* in order to insulate as, it were, a ray of light. In the principal experiment an image of the interference fringes was thrown on the slit of the spectroscope by the aid of the lens *f*; now, however, we increase

the distance between l and the plane of the slit. When, as in the experiment, the path of the light between the slightly silvered mirror and the lens is about 600 cm., we can by a displacement of 4.5 cm. (focal distance of the projecting lens 50 cm.), observe two sharp, coherent image points lying vertically above each other, in the image plane. The distance of the image points is a function of the wavelength. When they are projected on the slit of the spectroscope, the dependence of image distance and colour is directly observed. Fig. 3 represents a positive photo obtained with the tube of sodium under the said circumstances. The dark line on the left side and the dark line on the right side originate from one luminous point, the faint lines from the other. The difference of intensity of the lines was caused by this that the two luminous points were not thrown on the slit exactly under each other. The lines of Fig. 3 could not be observed in the neighbourhood of the absorption D-lines. In the lefthand part of the figure our lines are both curved upwards, in the righthand part both curve downwards. It is further noteworthy that the lines in the lefthand part intersect, whereas in the righthand part they diverge more and more from each other.

When we focus again the interference fringes upon the slit of the spectroscope we see the interference figure of Fig. 2 in the spectroscope.

Now it seems rather plausible that the existence of Fig. 2 is due to the shape of the lines of Fig. 3. We then expect a shift in the central part of the figure of the whole system of fringes, on the left upwards, on the right downwards. Further we expect interference fringes close together where the lines of Fig. 3 diverge greatly from each other, widely apart where the distance of these lines is small. No interference fringes are expected where the lines intersect. Thus the typical rhombic central part would arise in the figure.

It should still be pointed out that there is a narrow dark horizontal line in Fig. 2, this is due to a particle of dust on the slit. The vertical shadows in the lefthand part of the figure are the first indications of the absorption band spectrum of sodium.

Botany. — *"A method of recording growth under various external influences"*. By V. J. KONINGSBERGER. (Communicated by Prof. F. A. F. C. WENT).

(Communicated at the meeting of November 26, 1921).

Introduction. Since the researches of BLAAUW [1]) the problem of the influence of "stimulus" on growth has called the attention of the investigators. Hitherto BLAAUW's experiments have only been affirmed and extended by others on the base of the "photo-growth-reactions". BLAAUW's own method was always used i.e. determining growth by means of a horizontal microscope in weak red light, other external conditions being kept as constant as possible. That even this method is not quite perfect has been shown lately by Miss CL. ZOLLIKOFER [2]), who found that weak red light too exerts its influence upon growth.

Moreover the method of microscopical measurement would fail entirely for research concerning the influence of gravity upon growth as, e.g., with this method, accurate definition of growth on a clinostat is impossible.

Preliminary experiments, done with the same apparatus with which Miss ZOLLIKOFER has worked (magnifying power 90 ×) made it clear, that even errors, due to physical causes (vibrations, etc.) are not excluded. Since irregular values in measuring growth of an Avena-coleoptile were obtained, the latter was replaced by a micrometer-slide. Having focussed the microscope at a fixed line on this micrometer, the position of this line was determined every three minutes. Instead of an unvariable position, the following variations were found during half an hour:

$+12; +8; -15; \pm 0; +3; -10; +7; +15; -12; +7\ \mu$.

A small horizontal adjustment of the microscope, necessary at this enlargement at the slightest nutation of the seedling, led to errors varying from $+52$ to $-68\ \mu$.

The impossibility of measuring growth on a clinostat and in full darkness with this set of apparatus is clear. Moreover the larger

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[1]) A. H. BLAAUW. Zeitschr. f. Bot. 1914, **6**., id. 1915, **7**., Meded. v. d. Landb. Hoogeschool, Wageningen, 1918, **15**.

[2]) CLARA ZOLLIKOFER. Proc. Kon. Akad. v. Wet. Amst. Vol. XXIII. 1920, N°. **4**.

or smaller, but inevitable, observational errors must be added to the occurrence of physical disturbances.

These considerations have given rise to the planning of an autographical method.

In literature only one apparatus, used for these purposes, occurs; namely that of Bose and Das [1]), who transmit growth on a smoked plate, moved on by clockwork, by means of a lever-system at a magnification of 1000 to 10000 times. This crescograph can only record growth for a very short time, but the authors are not concerned with this factor, as the growth is so highly magnified. That observation-time is, however, of great importance may follow from the fact that the changes in velocity of growth, caused e.g. by light, are extended over a very long region of time. Further the test-plant is fixed on the elaborate lever-system by means of a thread, whereby the chance of physical disturbances is enlarged.

Moreover, this apparatus too is not adapted for use on the clinostat. So another kind of auxanometer was constructed in order to evade the errors mentioned above.

The principle of the apparatus. The growing test-plant closes a very weak electric circuit, by means of a refined contact, mounted on a micrometer-screw. This screw has a pitch of 0,5 m.M.; at its end is fixed a cogwheel with 100 teeth. The weak current, closed by the plant goes through a relais of high sensibility, which closes a stronger circuit. This stronger current moves on the cogwheel one, two or more teeth by means of an electro-magnet.

The screw is turned in this way $^1/_{100}$, $^1/_{50}$, etc. around its axis, the contact thus is raised resp. 5, 10 or more μ. The plant has to grow 5, 10 or more μ before it closes the circuit again.

Meanwhile a recording-pen on a rolling carriage-frame is moved on with a velocity of 1 m.M. a second by an electric clockwork (connected with a second-pendulum) along a non-moving recording-drum. The pen thus writes a straight line. At the instant of contact-making by the plant, the pen leaps back and arriving on its starting-point the drum is turned over a distance of 1,5 m.M. and directly the pen begins to write again with the same velocity. In this manner a series of lines will arise, while the length of each of them, measured in m.M. gives the time, in seconds, needed by the plant for growing 5, 10 or more μ. The curve, connecting the tops of these lines reproduces the course of growth. The machine records

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[1]) Sir J. C. Bose and G. Das. Proc. of the Royal Soc. of London, Series B, Vol. XC, 1919.

how long a time is wanted for a certain increase, in contrast with the "microscopical" method, where direct growth in a certain lapse of time is measured.

When this apparatus was practically constructed, my attention was called to a paper by BOVIE [1]), who has already put into practise the same device for an auxanometer. His method has several disadvantages. In the first place the linkage by means of an invar-thread is detrimental to the plant and in the secoud place the connection between plant and apparatus is by no means a rigid one. The chief disadvantage, however, lies in the fact that the plant itself closes the circuit, which effects the upward movement of the contact. This current, activating an electro-magnet, must be of a rather high voltage. Therefore it, inevitably, will emit sparks at the opening of the circuit. These sparks will burn the contact-metals, causing an inconstancy in the place of contact. BOVIE's auxanometer records on a drum, revolving at a velocity of 1 m. M. a minute. At each contact a pen makes a check on the drum. The distance between two checks thus corresponds to the time, wanted for a certain growth. Apart from the tedious counting of checks and measuring their distances, the slow movement of the drum leads to high errors in taxation.

Moreover, BOVIE didn't design his machine for our purposes; it meant to be a precision-machine for demonstration. Some years afterwards he describes [2]) a simplification of his apparatus, whereby, however, it has lost much of its accuracy.

To obtain a high grade of accuracy, many obstacles had to be overcome. The whole apparatus has been constructed by Mr. P. A. DE BOUTER, mechanic at the Botanical Laboratory of the University of Utrecht, from whose knowledge of engines the writer owes many ideas. The writer wants to render him his best thanks for the constant energy, with which he carried out his work.

The Auxanometer is mounted on a working-axis (**1**) (See fig. 1), 18 c.M. in length, that may be fixed on a clinostat-table (**2**). In order to revolve the plant vertically in regard to its revolving-axis, a side-axis (**3**) may be fixed on the working-axis (1). The test-plant, growing in a little pot of zinc (3 c.M. high and 4 c.M. diameter) may be fixed on a movable little table (4) by means of a handle (**6**) of the cover.

This handle (**6**) is fixed by a single screw (7), so that there is an unmovable connection between plant, pot and table.

[1]) W. T. BOVIE. Bot. Gazette, 1912. 53.

[2]) W. T. BOVIE. Am. Journal of Bot. 1915. 2.

This table (**4**) may be fixed on the auxanometer by the upward movement of handle (**5**). There is an opening in the cover for the plant and on that cover three little mirrors have been fixed for

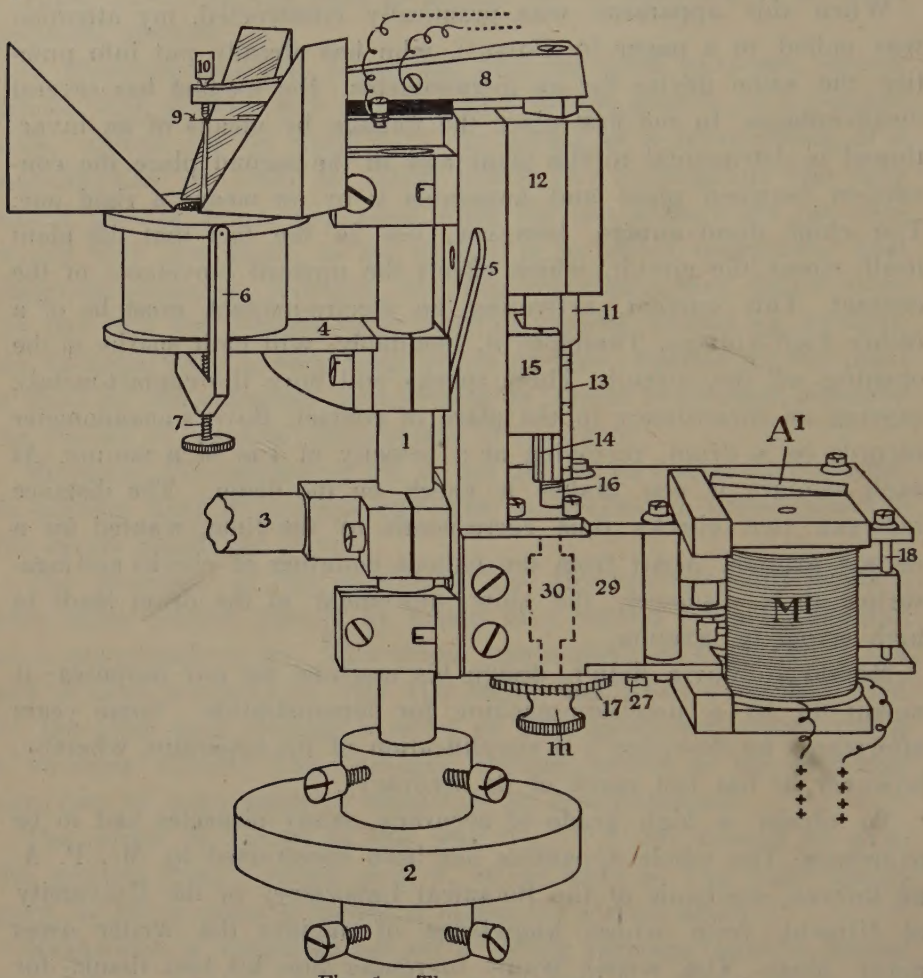

Fig. 1. The auxanometer.

exposition to light. The plant is drawn up exactly under the centre of the contact-device (**9**). On the brass piece (**8**) a small plate of brass has been mounted, isolated by ebonite, at which has been soldered a thin platinum strip (**9**). On this platinum strip lies a minute piece of polished gold, just opposite to a screw (**10**) in the brass piece (8) in the end of which has been soldered a fine point of platinum. This apparatus has been carefully made. It is an imperative condition that the platinum strip should yield very lightly to pressure and yet be elastic. If the screw (**10**) is at the minimum distance from the golden plate, without making contact, a weight of 2 mgr. on the platinum strip is sufficient (in inverse position) to

close the circuit. Furthermore the experiment must be stopped, as soon as the plant makes nutations, or grows in a wrong way. Therefore the platinum strip is narrow and allows but an excursion of the plant-top, less than 1 m.M.

The brass piece (**8**) has been mounted on a hexagonal brass prism (**11**) that runs true up and down in a well-fraised closed bearing (**12**). This prism (**11**) is linked by means of an internal strong spiral spring with a split-nut (**13**). In order to get a straight up and down movement this nut (**13**) runs by means of side-wings (**14**) in slits (**15**). The precision-micrometer-screw (**16**) runs in the nut (**13**). In the direction of the screw is saved a cylinder of larger diameter (**30**). Both screw-axis and cylinder (**30**) fit in a block (**29**). The turning of the screw-axis will raise split-nut (**13**), prism (**11**) and the brass piece with contact-device (**8**). At the lower end of the screw-axis under the block (**29**) a cogwheel has been mounted with 100 teeth (**17**) at such a rate that the contact is raised, when the wheel is turned in the direction of the sharper edges of the teeth.

In the beginning stage of the work, an energic electro-magnet

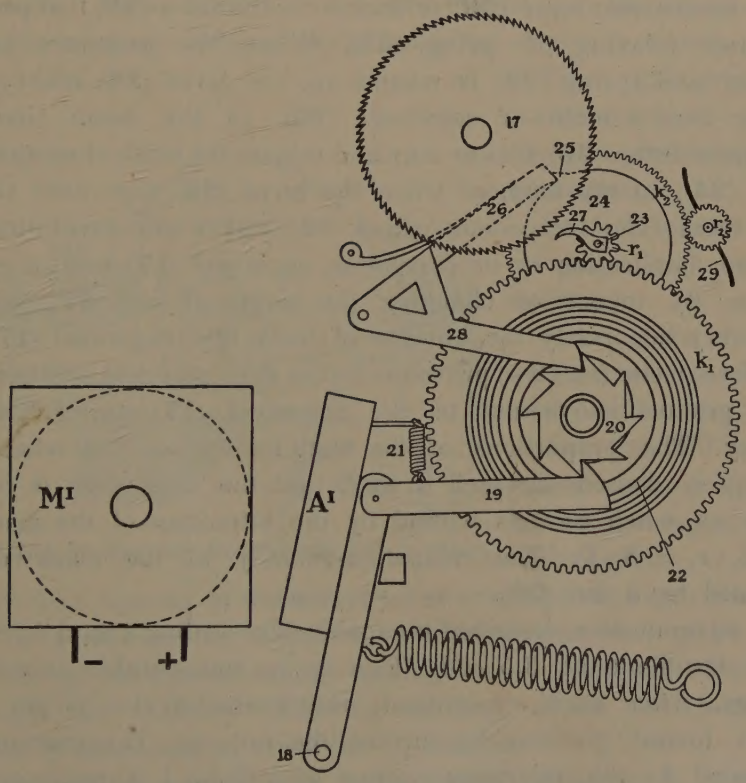

Fig. 2. Explanation in the text.

pulled on this cogwheel by means of an armature with a pawl. A spring drew back this armature just as far that, with the next closing of the circuit, the cogwheel should go on one, two or more teeth. This simple method, however, was not reliable, as the wheel sometimes turned too far, after the shocklike movement.

This error could only be eliminated by a rather complicated mechanism, ingeniously devised by Mr. DE BOUTER. Each time, when a current passes through the coil of electro-magnet (M^I) the armature (A^I) is attracted. This armature turns around an axis (**18**). (See fig. 2). On the armature is fixed a lever with a tooth (**19**), pressed against cogwheel (**20**) by means of spring (**21**). On the axis of cogwheel (**20**) is fixed a spiral-spring (**22**) which is wound up, when the armature (A^I) is attracted. This spring tries to relax itself on a little clockwork, consisting of some toothed wheels (k_1, r_1, k_2 and r_2). On the axis (**23**) of toothed wheel (k_2), however, is fixed an escapement-wheel (**24**) with a single tooth (**25**), which is held up by an escapement-lever (**26**). On the same axis (**23**) is fixed a little bolt (**27**) which, when revolving, should catch a tooth of cogwheel (**17**) of the auxanometer.

The escapement-lever (**26**) is one with the lever (**28**) that prevents the direct relaxing of spring (**22**). When the armature (A^I) is attracted and spring (**22**) is wound up, the lever (**28**) makes way, slipping over a tooth of cogwheel (**20**). In the mean time, the escapement-lever (**26**) makes way and relaxes the tooth of escapement-wheel (**24**), at the moment when the lever (**28**) slips over the top of the tooth. The escapement-wheel (**24**) makes one revolution, bolt (**27**) too, implicating in its revolution cogwheel (**17**) over a certain distance. By means of changing the length of bolt (**27**), one can adjust very accurately the number of teeth that cogwheel (**17**) shall turn. Two teeth ($= 10\,\mu$) proved to be the most practical arrangement.

The gradual movement of the cogwheel (**17**) warrants a high accuracy. The proportions of the teeth-number on the wheels (k_1) and (r_1) is chosen in such a way, that the clockwork is relaxed exactly as much as it is wound by the attraction of the armature (A^I). ($k_1 : r_1 = 8 : 1$). The relaxation-velocity of the clockwork is moderated by a fan (**29**).

The auxanometer described is capable of recording a total increment of the test-plant of 3,5 c.M. This is a considerable amount for seedlings. After each experiment, the contact-device is put down into its lowest position by turning the nut (m). The machine has been tested by the micrometer-screw of a Zeiss I A microscope. It has been placed in a room for constant temperature and shortly

will be mounted on the axis of a VAN HARREVELD'S clinostat. The electric connections will be secured, in that case, by sliding-contacts on the clinostat-axis.

As BOVIE (l.c.) has already mentioned, it is a great advantage that the test-plant may be as far as desirable from the recording apparatus. In our case, the latter has been placed in quite another part of the building.

The relais. As remarked above, a weak current is closed by the plant. In order to eliminate sparking at the interruption of the current, a condenser or a resistance (parallel to the contact) could be inserted into the curcuit. In the latter case, the resistance ought to be less than the atmospheric resistance, but large enough to prevent the relais to react. It can, however, be eliminated more safely by using a current of very low voltage, led through a fit relais. This relais has been found in the form of a galvanometer, with two coils of 4000 windings each. On the mirror of this galvanometer an iron electrode (**31**) (see fig. 3) is fixed, on which two platinum tips have been soldered. When the mirror (with the electrode) turns, these platinum tips are moved into two small cups, filled with mercury (**32**), whereby a second circuit is closed.

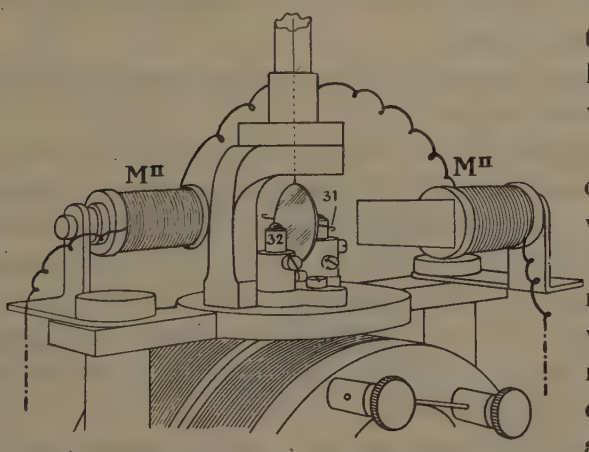

Fig. 3. The galvanometer, transformed into a relais.

This galvanometer, transformed into a relais, has the following advantages:

First. It has an extraordinary high sensitiveness.

Second. As there is no iron pith, in contrast with other kinds of relais, the self of the current is so small, that at the opening no spark will occur.

Third. The turning of the mirror takes a rather long time ; the relais having a great inertia. Short current-pulses, as will result from vibrations, do not possess enough turning-power, to make the electrode (**31**) reach the mercury (**32**). In order to obtain this result, the circuit is to be closed at least for $^1/_5$ second.

In this simple method the influence of vibrations is efficiently eliminated.

The mirror gives a full excursion at a current, obtained from an accumulator (I) (see fig. 4) and diminished by a resistance (II) of several hundreds of Ohms. The intensity of the current is about 1 milliampère. This current, which affects the galvanometer (III) (·····) closes a second circuit (----------) derived from the same accu. This circuit passes through a second relais (IV). There a third circuit (·—·—·—·—·—·—·) is closed. The current for this circuit (and for others, that will be mentioned below) is derived from the central-net. The voltage (220 volts direct current) is diminished by resistance-lamps. These lamps pass ± 0,6 amp.

This current has three things to do:

First. It affects a third relais (V) which closes the circuit (+++++++++++++++) activating the auxanometer-magnet (M^{I}). In this way, the contact-device is raised.

Second. When the platinum-tips of the galvanometer penetrate into the mercury, a rather large power is required to turn the mirror back, as the surface-tension of the mercury is considerable. The terrestrial magnetism doesn't generate enough power, to warrant this safely. Therefore two little magnets (M^{II}) have been placed perpendiculary to the iron rod (**31**). When the current affects these magnets, the electrode will be pulled energetically out from the mercury; the swinging movement is to be damped by strips of paper, glued on the magnet-piths.

Third. It has to activate magnet (M^{III}) of a turn-over switch (VII) which is an essential part of the:

Recording-Apparatus (see fig. 4 and 5). By means of a simple clockwork the pen, mounted on a carriage-frame (**34**), is drawn along the paper. Two electro-magnets (M^{V} and M^{VI}) regulate this movement. The carriage (**34**) is connected, by means of silk-threads, at one end with a wooden cylinder (**33**), at the other end with a weight (**35**). The cylinder (**33**) is mounted on the same axis as the cogwheel (**36**), which turns to the right, when the metal device (**37**), with a pawl (**38**), moves down.

Every second a circuit (·····) is closed by a second-pendulum (IX) which affects a relais (VI). This relais activates by circuit (·+·+·+·+·+·+·+·+·+·) magnet (M^{V}) which attracts the device (**37**) as its armature (**40**) is mounted on this device.

With device (**37**) pawl (**38**) is pulled down and the latter involves cogwheel (**36**) in its movement; i.e. cogwheel (**36**) is turned one tooth and cylinder (**33**) winds up the silk-thread; the carriage (**34**) advances 1 m.M. A spiral-spring (**42**) pulls back the device (**37**) till it is arrested by a metal block (**43**), just when pawl (**38**) catches

the next tooth of cogwheel (**36**). A contra-pawl (**41**) prevents the rolling back of the carriage during this moment; this pawl (**41**)

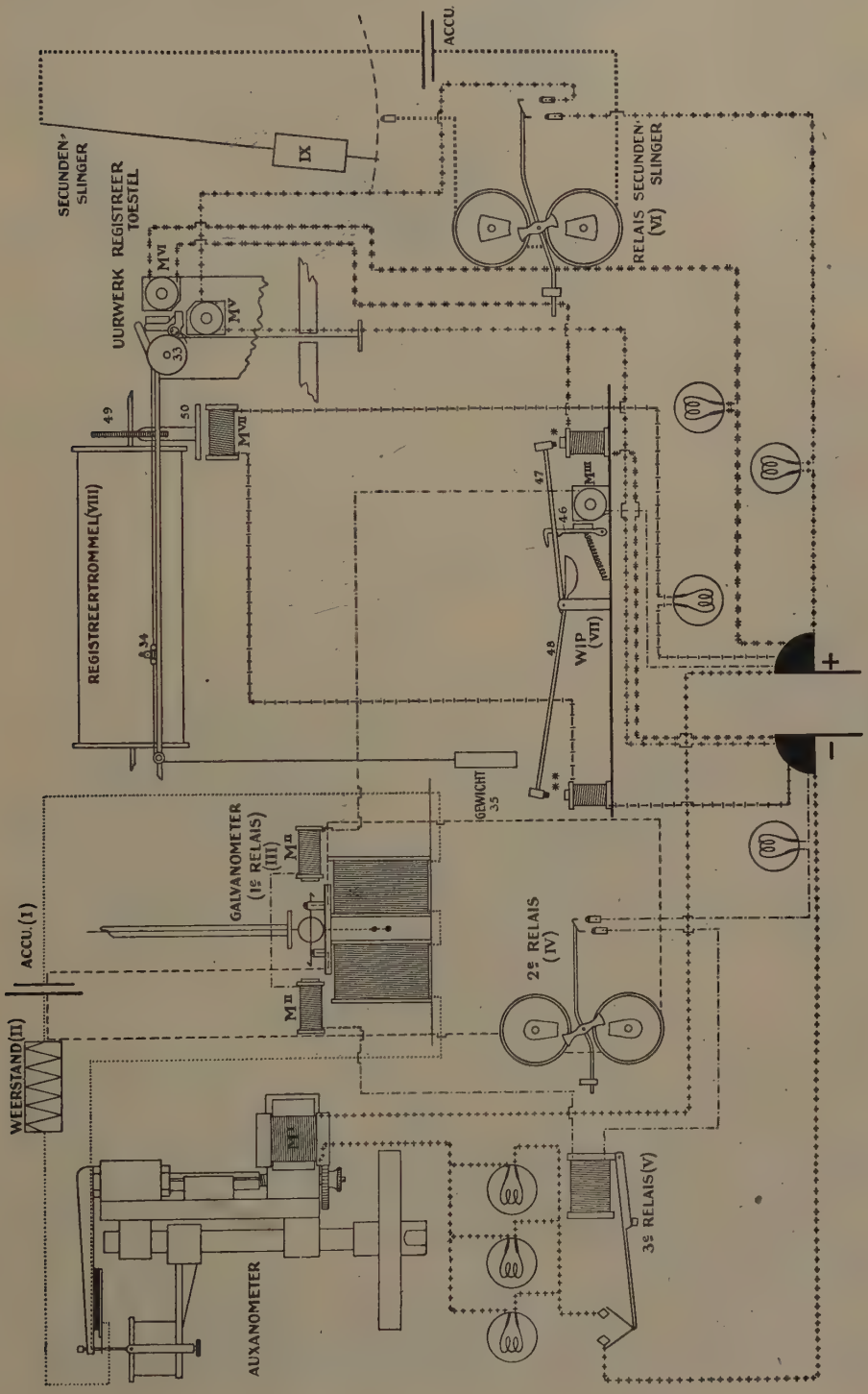

Fig. 4. Explanation in the text.

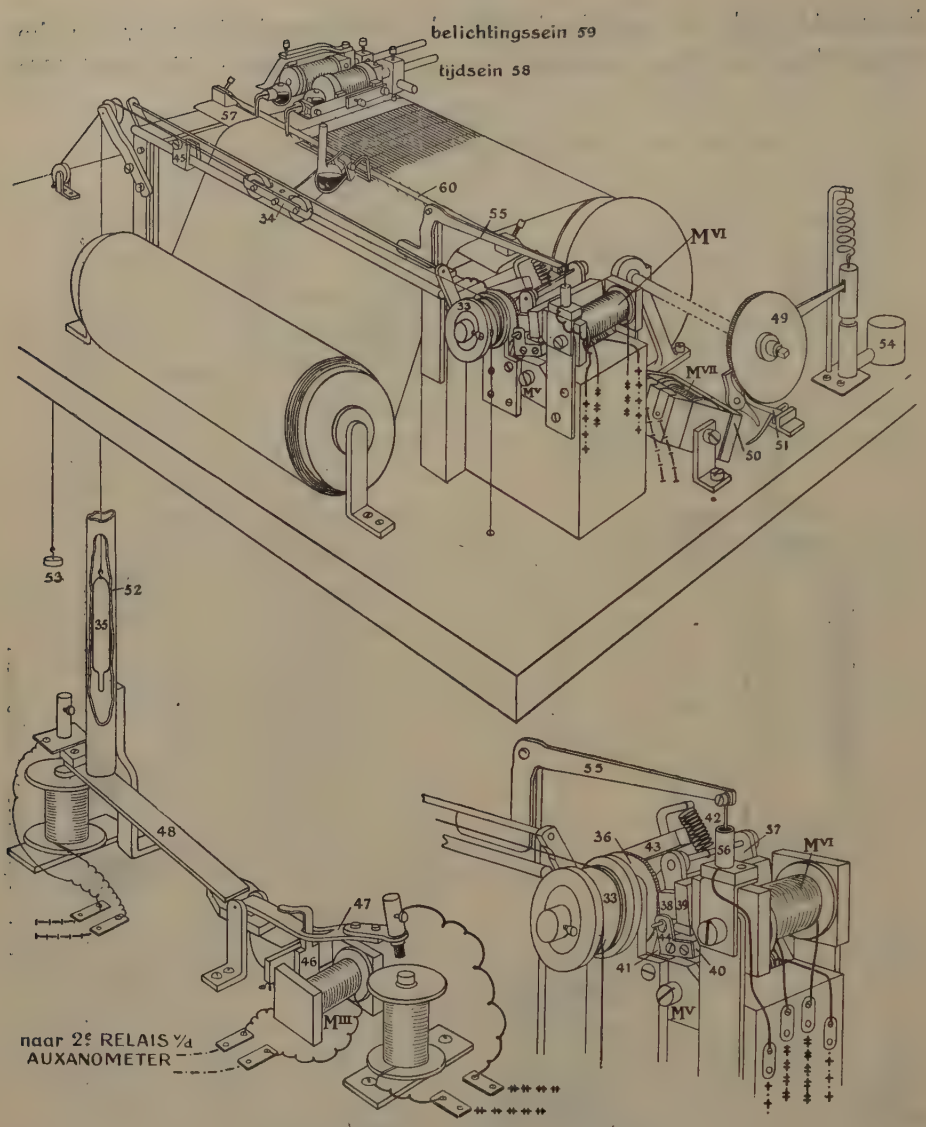

Fig. 5. The Recording Apparatus.

projects with a little bar through a metal ring (**44**) of device (**37**). On the same device is fixed a second armature (**39**), belonging to magnet (M^{VI}). When a current activates this magnet, the whole device (**37**) is attracted to the right. The contra-pawl (**41**) is also attracted, as it projects through ring (**44**). In this way cogwheel (**36**) and cylinder (**33**) come free and carriage (**34**) rolls back, pulled by the falling weight (**35**).

The magnet (M^{VI}) will be activated by the switch, for the circuit closed by the second relais (IV) passes through the coil of magnet (M^{III}). The armature (**46**) of this magnet is attracted and the support

of lever (**47**) is taken away. This lever closes at *) the curcuit (++++++++++++), which affects magnet (M^{VI}).

When weight (**35**) has fallen down straight (in a metal tube (**52**) it presses down lever (**48**), that lifts by its shorter arm lever (**47**) and opens the circuit of magnet (M^{VI}). Instantaneously the device (**37**) is relaxed from magnet (M^{VI}) by a spring (invisible on fig. 5) and the second-pendulum begins again to draw up the carriage.

As the drum is a non-moving one, a second line would be drawn on the same spot as the first. Therefore the fallen weight closes a circuit at **) (—|—|—|—|—|—|—|—|—|—), which affects magnet (M^{VII}).

Armature (**50**) is attracted and a pawl with it. The pawl pulls on cogwheel (**49**) that is mounted on the axis of the drum (VIII).

The latter is turned 1,5 m.M. The new line thus will be drawn 1,5 m.M. from the preceeding one. As the drum has a great inertia and the system was moved on some times more than agreed with one tooth of the cogwheel (**49**), the movement is moderated by an oil-pump (**54**), whereas a contra-pawl (**51**) prevents the turning back of the drum. When weight (**35**) rises again, the circuit is opened, as lever (**48**) is lifted by a weight on its shorter arm.

In this way a registration has been obtained, at which the length of each line expresses in m.M. the time in seconds which the plant needs for an increase of 10 μ. As the lines are drawn 1,5 m.M. apart, 15 c.M. of the paper corresponds to 1 m.M. growth (100 lines).

Several precâutions have been arranged to secure a safe record. In the first place, the record has to be stopped automatically, when the plant grows in a wrong way, i.e. when it ceases to make contact. In that case, the carriage (**34**) is drawn up entirely and pulls at the end on lever (**55**). The current, which affects magnet (M^{V}), passes through this lever (omitted in fig. 4) and a small cup (**56**) with mercury. When the lever is lifted out of the mercury by the pressing carriage, the circuit is opened.

To obtain a record over a long lapse of time, the use of several yards of paper is necessary. These have been rolled on a second drum and laid over a metal plate (**57**). So the pen has to write on the same level, while the drum (VIII) increases in bulk.

In order to get a straight abscissa the carriage has to stop always at the same starting-point. This is obtained by a metal block (**45**) and a small plate, connected with cylinder (**33**). The under-side of the table stops this plate, when the carriage reaches block (**45**). A second weight (**53**) has to pull the carriage, when the power of weight (**35**) is broken on lever (**48**).

To prevent the tedious burning of the contacts on the switch

(VII) the principle of coil-contacts has been put into practise; the self of the current blowing away the opening-spark.

The space-marking on the paper is arranged, by using the fact that the ink (a solution of eosin in water and some glycerin) remains wet for some time. On a metal strip (**60**) stiff bristles have been glued, at distances of 5 mM., which drag over the wet ink and make checks in the lines.

An electro-time-sign (**58**) draws a straight line parallel to the abscissa of the record and makes checks e.g. every 10 minutes. As the lines are drawn 1,5 mM. apart, the distance in mM. between two time-checks (devided by 1,5) gives the increase in 10 minutes. As it may happen that a check falls just before the carriage will be driven back, and the next check just after this moment, the maximum error that may occur will be 20 μ. In this way a simple method has been found, to compare the results with those of other investigators.

Lastly a second time-sign (**58**) can be put into action from the experiment-room. One can, by pressing a bell-push, put a point on the paper at the moment, when light is dosed or when the auxanometer has been put on the clinostat, etc.

Next figure 6, a little reduced in size, gives the record of 1 mM.

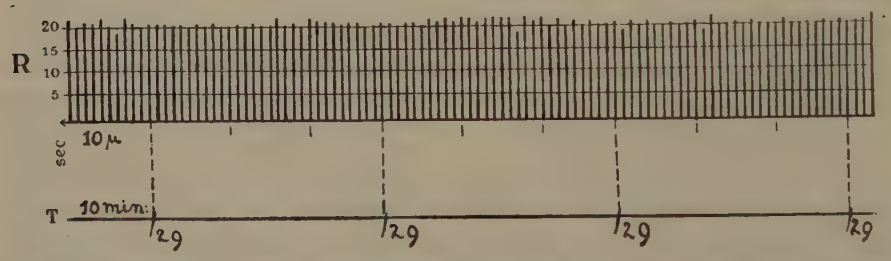

Fig. 6. Record of 1 mM. growth. Each line at R gives the time needed for 10 μ growth; T = time-line; every 10 minutes the increase is constantly 290 μ.

growth, belonging to a paper of 4,05 M. in length, containing the record of 27 mM. growth, in the dark.

Utrecht, November 1921. *Botanical Laboratory.*

Anatomy. — "*On the development of the sternum in reptiles*". By CHR. VAN GELDEREN. (Communicated by Prof. L. BOLK).

(Communicated at the meeting of June 25, 1921).

Comparative anatomy generally indicates as sterna those parts of the skeleton that lie in the ventral median line of the trunk-wall. In the reptiles, which will be discussed in this paper, we have to distinguish between episternum and sternum s. strictiori. The sauria and crocodilia possess a sternum s. str.; with the exception of the rhiptoglossa they have an episternum besides. Considering the uncertainty existing about the development of the above named sterna, for the present only the histological build of the adult reptiles may be named as the only difference between the two. The episternum consists of bone, the sternum s. str. of cartilage, which is often calcified. This communication only concerns itself with the development of the sternum s. str.

We owe to RATHKE the first data about the development of the sternum, in reptiles as well as in birds and mammals [1]). He found in Lacerta agilis, that in early embryonic stages the sternum consisted of two entirely separated parts. Each part was a strand of tissue consisting of a dense mass of cells, which connected the ventral ends of the future vertebro-sternal ribs. Afterwards the two sternal parts fused in cranio-caudal direction. In Anguis fragilis, on the contrary RATHKE found that the sternum developed apart from the ribs. Later [2]) on RATHKE published his experiences in embryos of crocodiles. They were entirely in accordance with those in Lacerta. One cannot conclude from RATHKE's works whether he saw any fundamental difference between the two ways of development of the sternum described above, separated from the ribs in anguis connected with the ribs in lacerta and crocodilus; neither can one conclude whether in his opinion there is a genetic relation between the sternum and the ribs in lacerta and crocodilus, in other words, whether the sternum is a product of the ribs inserted into it.

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[1]) H. RATHKE. Ueber den Bau und die Entwickl. des Brustbeines der Saurier Königsberg 1853.

[2]) H. RATHKE. Ueber den Körperbau und d. Entwickl. der Krokodile. Braunschweig 1866.

Götte [1]) examined the development of the sternum in Cnemidophorus spec. and in Anguis fragilis. In Cnemidophorus the first formation was paired and consisted of a triangular widening of the ventral end of the first (future) vertebro-sternal rib. This primitive formation developed caudally only so far as further ribs attached themselves to it, so not independently. Moreover Götte thinks it probable that the last cervical rib, which in the further development is more and more removed from the sternum, has also taken part in the first formation of the sternum. In Anguis the sternum was formed out of the widened end of the first rib, which soon after this was loosened from the sternum.

Apparently the embryos examined by Rathke were too old for the purpose. The results of Wiedersheim's [2]) examinations of Lacerta and Anguis agree very well with Götte's experiences. Only he thinks it probable that also the last but one cervical rib takes part in the formation of the sternum. Also in crocodilus biporcatus the sternum, according to Wiedersheim, is formed by the ribs.

Schauinsland [3]) describes the sternal formation in Sphenodon, first connected only with one rib, afterwards with three. Out of these one has to think the sternum has been formed. And, lastly, according to Bogoljubski [4]) the paired first sternal formation in Lacerta and Anguis is formed without any original connection with ribs, is therefore an autochthonie formation. In short, according to the generally prevailing opinion the sternum of the reptiles is formed out of the ribs [5]). Only Bogoljubski supposes the sternal formation in the reptiles to be autochthonic.

It is advisable to mention here that there are some more theories on the development of the sternum of higher amniota, the mammals. According to Paterson [6]) the first formation of the sternum consists of an unpaired, dense mass of mesoblastcells, lying in the median line. Later on one finds two sternal bands because the median part has become poorer in cells. The relation between ribs and sternum is secondary, on the other hand there is a primary connection between the median formation and the shoulder girdle. Whitehead and Waddel [7]) suppose the mammaliam sternun to be built out of

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[1]) A. Götte. Archiv. f. mikrosk. Anat. Bd. XIV, 1877.

[2]) R. Wiedersheim. Das Gliedmaszenskelett der Wirbelthiere. Jena, 1892.

[3]) H. Schauinsland. Archiv. f. mikrosk. Anat. u. Entw.gesch. Bd. LVI, 1900.

[4]) S. Bogoljubski. Zeitschr. f. Wissensch. Zool. Bd. 110, 1914.

[5]) O. Hertwig's Handb. d. vergl. u. experim. Entwickl.lehre. Jena.

[6]) A. M. Paterson. Journ. of Anat. and Physiol. Vol. 35. 1900.

[7]) R. H. Whitehead and Waddel. Americ. Journ. of Anatomy. Vol. 12, 1911.

two autochthonic sternal bands, and moreover out of a third, also autochthonic, median formation. This median formation is independent of the sternal bands, and also of the shoulder girdle. This appears most clearly by the fact that the median formation is present also there where the clavicula is absent, so where the shoulder girdle does not reach the sternum. HANSON [1]) sees in the median formation part of a large blastema with the shape of a horse-shoe, out of which the two shoulder girdles and the cranial part of the sternum are formed.

All biologists who examined the development of the reptilian sternum agree that the sternal bands are fused after their becoming cartilaginous. And after that, calcification may follow.

In order to get an opinion founded on personal observation I examined a number of sauria-embryos. My experiences may follow here.

For this examination I had at my disposal a dozen embryos of Gongylus ocellatus, sectioned into series, and two of Ptychozoon homalocephalum. Besides, transversal series were made of some

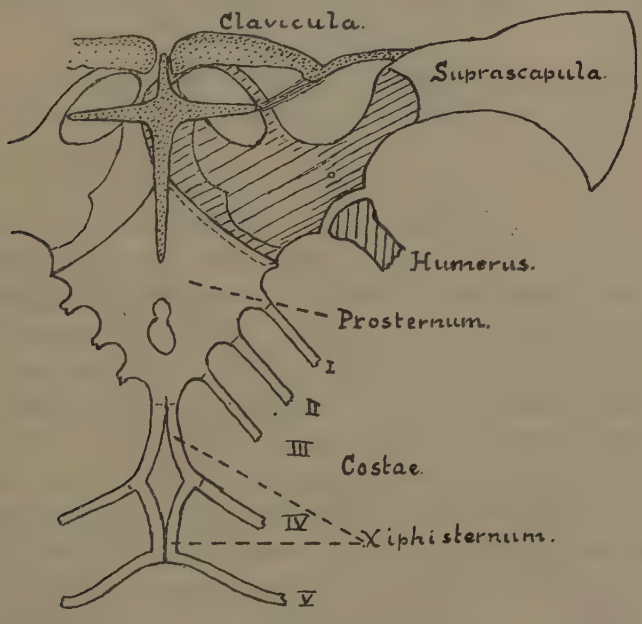

Fig. 1. Sternum and shoulder girdle of Lacerta agilis.

sixteen embryos of Lacerta agilis. All series had been sectioned 10 μ thick, most of them had been coloured with haematin. The development of the sternum could be very well observed in this series of

[1]) F. B. HANSON. Americ. Journ. of Anatomy. Vol. 26, 1919.

embryos of lacerta, which was sufficient in all respects. Therefore I shall begin with a description of my experiences with lacerta.

For further elucidation the shoulder girdle and sternum of an adult lacerta agilis have been drawn from nature in diagram 1. The whole complex has been drawn in a plane. One may observe that at both sides of the prosternum three ribs are inserted by syndesmoses, and that the xiphisternum supports two ribs. It would be better to speak of two xiphisterna here, as at the level of the fifth rib there is only a syndesmotic connection in the median plane. Prosternum and xiphisternum are also connected by syndesmoses Coracoideum (hatched) and sternum are connected by a diarthrosis; the coracoideum fits in a groove, sulcus articularis coracoideus, of the sternum. Clavicula and episternum, which are further left out of consideration, are dotted.

In the prosternum we find a fontanel closed by membrane.

The youngest embryo at my disposal, Lacerta ag. D. (N.T. [1]) about 22) had not yet any sternal formation. Neither was there anything to be found of the shoulder girdle as yet. Only in the extremity a thickening of the mesenchym, the first formation of the humerus, was found.

The next embryo, Lacerta ag. S. (N.T. about 24) differs from the preceding one in so far that the formation of the shoulder girdle, continuous with that of the humerus, is visible. As this shoulder girdle is as yet very vaguely outlined, one can hardly distinguish any shape in it, at most a ventral coracoidal part and a dorsal scapular part. Now if one looks more in a caudal direction, one finds in the sidelong wall of the trunk a densening of the mesenchym, which is unconnected with any other skeleton-formation. This is the first formation of the sternum, which as yet consists exclusively of blasteme, dense mesenchym. In diagram 2 a transversal section has been drawn, in which there is a sternal formation. The section is not quite transversal; in the lower part we see the humerus, in the upper part radius and ulna. Still the left hand as well as the right hand sternal formation are present in the section. This proves that the sternal formation has already been extended in cranio-caudal direction. It is not possible to demonstrate in pictures of transversal sections the independences of the sternal formation of the humeral zone (in casu the coracoideum). As will appear later on the more obliquely sectioned series of Gongylus ocellatus are better suited for this purpose.

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[1]) Normentafel. Lacerta agilis von K. Peter.

The next stage is represented by the embryos Lacerta ag. E and F (N.T. about 26), which show nearly the same stage of development. The outline of the shoulder girdle has become clearer. The coracoidal and the scapular part can very well be recognized. In

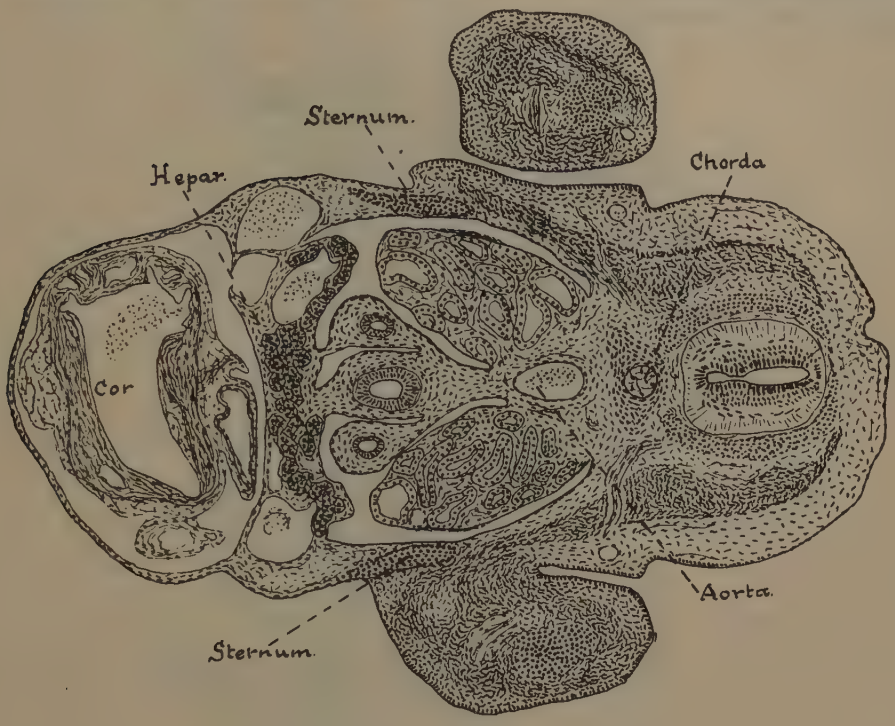

Fig. 2. Lacerta agilis S. transversal.

the humeral diaphysis praechondrium is found. In comparison with embryo D there is a further progress in the increased size of sternum and shoulder girdle. Sternum and shoulder girdle have grown in each other's direction. This has caused the layer of undensened mesenchyme, ab origine found between them, to become less clear.

A considerable progress in development may be stated in the embryo Lacerta ag. I (N.T. about 28). In the humerus we here find for the first time cartilage. In their further growth sternum and coracoideum have come so near to each other that they appear together in transversal sections. It has become almost impossible to outline them clearly with respect to each other. In the accompanying diagram we see an only slightly lighter zone of partition. If the embryos described above had not been examined, the sternum would, on the ground of this embryo, certainly have been declared to be a product of the coracoideum, and this stage would have been interpreted as the first stage of the sternum being cut off its matrix, the coracoideum. The ribs have approached the sternal formation

up to some distance, so that the length of the sternal forman to can now also be indicated with respect to the ribs. In caudal direction the sternum reaches to the level of the third rib. Between the sternal formation and the ventral ends of the ribs we find everywhere loose mesenchyme.

Fig. 3. Lacerta agilis I. transversal.

In the embryo Lacerta agilis K (N.T. about 29), which is only slightly older than the preceding one, again a clear partition of coracoideum and sternum is present, a fact which strikes one also in studying the embryo Lacerta ag. G., which represents the same stage of development as the embryo K. The zone of partition, here again present, is nothing else than the formation of the later sterno-coracoidal articular cavity. Both the embryos G and K have a paired sternal formation, reaching caudally to the level of the third rib, and separated from the ribs by loose mesenchyme.

In the embryo Lac. agilis H (N.T. about 30) a considerable progress is noticed. This progress concerns the form of the parts of the skeleton as well as the histological differentiation. In the vertebral arches we find cartilage, round the cartilaginous humeral diaphysis we find a thin coat of perichondral bone. Diagram 4 brings us one half of a transversal section in which the ventral end of the first thoracic rib (the first future vertebro-sternal rib) is found. The purpose of the

diagram is to demonstrate the relation between the first rib and the sternal formation. In this embryo, which has reached a rather advanced stage of development, as is proved by the advanced histological differentiation, still all connection between sternum and ribs

Fig. 4. Lacerta ag. H. transversal.

is wanting. The second and third ribs namely behave exactly in the same way as the first.

The next embryo, Lacerta ag. J. (N.T. about 31) is distinguished from all those described above by the possession of a sternal formation, which extends in a caudal direction past the third rib. The sternal formation here consists of one larger cranial part, in which praechondrium is found and with which three thoracic ribs are connected by moderately dense mesenchym, and a small caudal part, which has the appearance of an offshoot of the former. This offshoot is still purely dense mesenchym and ends at a short distance

of the ventral end of the fourth rib, separated from the latter by loose mesenchym. At the description of the embryo Gongylus ocell. L. one finds entirely similar relations made clear by two frontal sections (diagr. 6).

Embryo Lacerta ag. L. (N.T. about 31) shows little progress as compared with embryo J. The principal point is that the two sternal ridges are here connected with three ribs by entirely densened mesenchym. The relation between the fourth rib and the sternum has not undergone any change. The extension of the praechondrium in the sternal formation has increased. The caudal offshoot is still entirely free from praechondrium. If one compares diagrams 2 and 4, one sees that the sternum in diagr. 4 is still found at the same place as in diagr. 2 viz. in the lateral trunk-wall. Shifting towards the ventral median line has not yet taken place. In the older embryos following, this shifting becomes clearer and clearer. One might suppose some connection to exist between this shifting and the longitudinal growth of the ribs with which the sternal formation is now connected. It seems to me better to take the relative decreasing of the heart-bulge for the only cause of this. For though in Anguis the sternum and the only vertebro-sternal rib are soon definitively separated (WIEDERSHEIM), still the sternal halves shift towards the median line to grow into one whole there.

In embryo Lacerta ag. N. (N. T. about 32) we find a beginning of important phenomena of development. In the first place a densening of mesenchym has appeared here between the end of the fourth rib, and the still blastematical end of the sternum. Here is as yet no question of complete joining, as the intermediate zone has not yet reached the same state of density as the sternal formation. The ventral end of the fifth rib, the last of the future vertebro-sternal ones, is situated thirteen sections caudally (130 μ) to the insertion of the fourth rib to the sternum in the straight ventral muscle.

In the second place I have to mention here that, in spite of its further development in comparison with embryo L, the sternal formation does not reach caudally past the insertion of the fourth rib.

From various circumstances it appears that embryo Lacerta ag. N. (N. T. ab. 33) is further developed than the preceding are. As regards the sternal formation, here too the fourth rib is connected with the blastematic caudal end of the sternum by completely densended mesenchym. The end of the fifth rib is situated only eight sections (80 μ) caudally to the insertion of the fourth rib to the sternal band. Consequently the fifth rib has been lengthened, and has grown in the direction of the sternal formation. But, conversely,

the sternal formation has not been lengthened past the fourth rib in the direction of the fifth. I think I have to conclude from this that the autochthonic sternal formation does not reach further caudally than up to the insertion of the fourth rib. The two sternal bands in this embryo have come cranially within a very short distance of each other. Neither here nor in any of the younger embryos there could be observed anything like a median formation that was also to grow into one whole with the sternum. Still the sternal ridges are here found immediately before the beginning of the growing together in the median line, as appears from the following embryo.

I found a somewhat older stage in the embryo Lacerta ag. Q. (N. T. about 33). The sternal bands, which here were cartilaginous, had already fused in the most cranial part. Caudally to the later breastbone-fontanel as yet no joining had taken place in the median line. All five ribs were joined by means of cartilage to the sternum, which was also cartilaginous; so they formed together one large continuum of cartilage. Moreover the syndesmoses are wanting, which in the adult lizard separate the xiphisterna from the prosternum.

In the embryo Lacerta ag. Q. (N. T. ab. 33—34) the sternal bands have fused cranially, as in embryo P; then follows the region of the breastbone-fontanel; still more caudally, on a level with the insertion of the third rib, the sternal bands are again situated close to each other. A thin layer of blastema proves that no fusion has as yet taken place here. Caudally to this part the sternal ridges diverge, never to reach each other again (c. f. diagr. 1).

Still completer is the fusion of the sternal bands in embryo Lacerta ag. R (N. T. ab. 34—35), while in embryo Q there was only a blastematic connection in the median line, in embryo R a cartilaginous connection is formed caudally to the fontanel. Moreover the two xiphisterna have joined on the level of the fifth rib. So in this embryo, the oldest examined by me, the adult form has been reached, at least in the main. A difference is still formed by the absence of all syndesmoses. They have to be formed secondarily in places where cartilage existed first. Therefore these syndesmoses have no morphological value; they are not lines of division to which, strictly taken, any importance may be attached. They have only a mechanical importance.

Of Gongylus occellatus I had eleven embryos at my disposal. Older stages, like the embryos P, Q and R of lacerta were wanting. Neither did I possess an adult specimen, nor an image of the adult sternal apparatus. So I can only give a few data established in

literature concerning the structure of the latter In. Gongylus, as in Lacerta, three ribs are fixed to the Prosternum. To the xiphisternum, too three ribs are fixed (HOFFMANN [1]), PARKER [2]), FÜRBRINGER [3]). With the exception of this difference, which had no importance for the study of my material, the relations agree with those of Lacerta.

The embryo Gongylus ocellatus F. and G. agree in their stage of development with the embryo S. of Lacerta. They contain a blastematic sternal formation, which is clearly unconnected with the formation of the coracoideum.

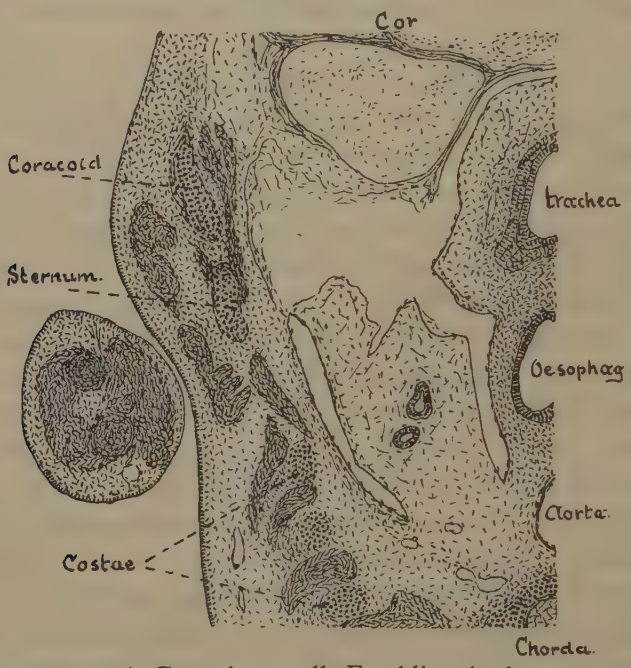

Fig. 5. Gongylus ocell. F. obliquely frontal.

Thanks to the oblique direction of sectioning it was possible to draw the abovesaid relation between sternum and coracoid in diagr. 5, which represents part of such an oblique section. Also by the peculiar direction of sectioning, in this section the sternum is situated dorsally to the coracoideum. Between the two we find a small layer of loose mesenchym. In the diaphysis humeri we find already some praechondrium.

A following stage is represented by the embryos Gong. ocell. A and B. They are similar to the embryos E and F of lacerta. The

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[1]) C. K. HOFFMANN in BRONN's Klassen u. Ordn. des Thierreichs. Reptilien.

[2]) W. K. PARKER. Monogr. on the Struct a. developm. of Should. g. and Stern. Ray Soc. 1868.

[3]) M. FÜRBRINGER. Jenaische Zeitschr. Bd. 34, 1900.

sternal formation is not clearly separated from that of the coracoideum; the cause of this state of things lies in the fact that the two have grown in each other's direction. The humerus is here for the greater part built up of cartilage. The ribs are still separated from the paired sternal formation by loose mesenchym.

Still further developed are Gong. ocell. C and D. They have a breastbone formation that is clearly separated from the coracoideum by a thin dividing layer, the formation of the diarthrose. The stage in which the division of sternum and coracoid was almost impossible, is over here. The three ribs, which end within a short distance of the sternum are still entirely unconnected with A. The sternal formation does not reach further caudally than the third rib.

The embryos E and J of Gong. ocell., too represent one and the same stage. As a basis for description I take embryo E. Sternum and coracoid are definitively separated. Three ribs are connected with the praechondral prosternum by mesenchym that is moderately dense.

A caudal blastematic offshoot of the sternal ridge grows in the direction of the fourth rib, but is still entirely unconnected with it. As some few sections were wanting I could not with certainty fix the relation between all prosternal ribs separately and the sternum. Undoubtedly the above said observations can be generally applied, as is proved by Gong. ocell. I. In this embryo there is one and the same relation between each of the three ribs and the sternum, viz. that of a still less clear connection than in embryo E. The caudal offshoot of the sternal formation, too, is smaller here. On the other hand the relation towards the coracoid is the same.

Embryo Gong. ocell. L. corresponds with embryo lacerta J. On both sides three ribs are joined to the sternal formation by completely dense mesenchym. An offshoot grows in a caudal direction towards the fourth rib, as is shown in diagr. 6. In this diagram two consecutive sections out of this series have been partly drawn. The direction of sectioning was here frontal to the thorax. The sternal formation has here to a large extent shifted in medio-ventral direction

In embryo K, lastly, the one furthest developed, the fourth rib, too, is joined blastematically to the sternum. For want of older embroys the development of the xiphisternum in Gongylus could not be followed any further. From what precedes it appears that the results obtained in Gongylus are a confirmation of the experiences in Lacerta.

Finally, I had an opportunity to study two series of young embryos of Ptychozoon homalocephalum.

In the embryo Ptychoz. hom. A we find a very early stage. A blastematic paired sternal formation, unconnected with any other skeleton-formation, is found in the lateral trunk-wall.

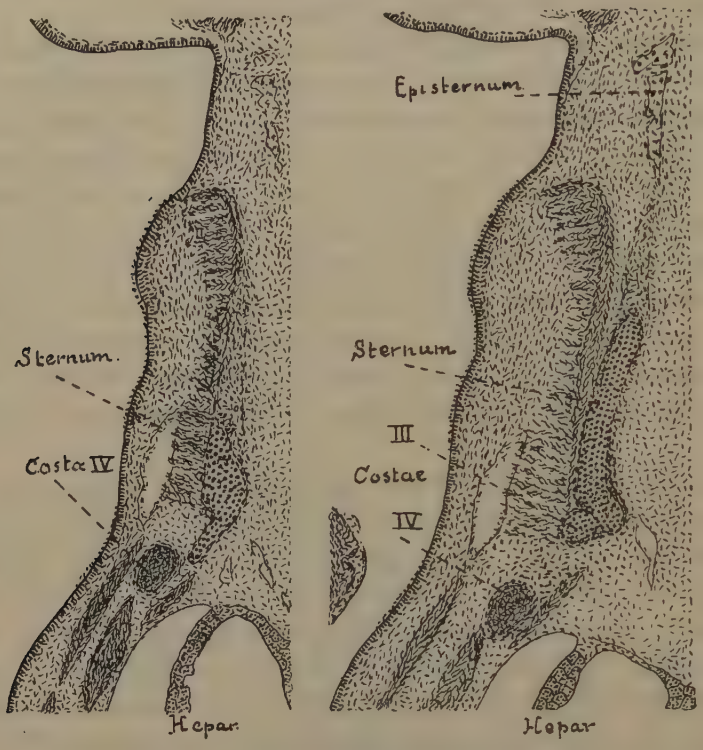

Fig. 6. Gongylus ocell. L. frontal.

If one sees the smallness of the ribs, and that in the humerus no cartilage is as yet present, one may conclude that the maximal approach of sternum and coracoideum has not been reached by a long way, in other words that the zone of division does not represent the formation of the articular cavity. So this embryo corresponds on the whole with embryo S. of lacerta.

The embryo Ptychoz. hom. B is much older. On both sides one finds a sternal formation to which three ribs have been joined.

In the preceding words my experiences in studying some thirty embryos were rendered separately. We shall now consider what conclusions they enable us to draw.

In the first place: the youngest formation of the sternum is paired and autochthonic; in so far I quite agree with Bogoljubski. If one has come to this conclusion, one has to ask oneself this question: What part of the definite thorax-skeleton was formed out of this

autochthonic sternal formation, or in other words: where are the divisions between the autochthonic breastbone and the ribs situated? With regard to the first three vertebro-sternal ribs the answer is easy. Here the abovesaid divisions correspond with the definitive syndesmoses sterno-costales. With regard to the fourth and fifth ribs, in order to get certainty there, one has partly to take for basis magnitudinal relations such as are represented in diagram 7. In diagr. 7*d* the little crosses indicate the situation of the definitive sternocostal syndesmoses. It appears from the diagram that the place where the fourth rib has placed itself against the autochthonic sternal band (7*c*) is not the same as that where later on the syndesmoses sterno-costalis IV is found, but that the latter is situated at the place of the later division between prosternum and xiphisternum. One

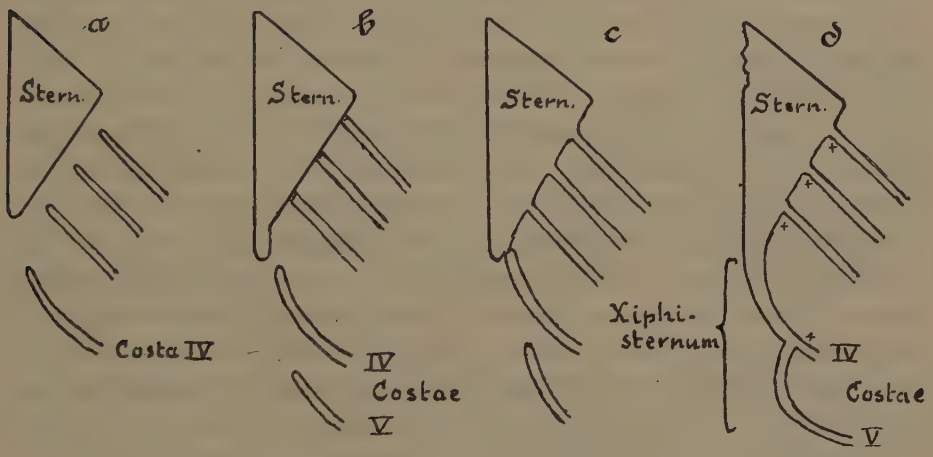

Fig. 7. Lacerta agilis. Outlines of the development of the Sternum.

may be reminded again of the fact that all syndesmoses in this region of the thoracic skeleton have been formed secundarily in places where first (7*d*) there was cartilaginous continuity. A consequence of this is also that one will never be able to tell exactly where in adult reptiles the autochthonic sternum ceases, where the ribs begin. I never saw the autochthonic sternal formation reach further caudally than the insertion of the fourth rib. On the other hand I did not see either that the fifth rib placed itself against the fourth, while the latter did not yet form a cartilaginous continuity with the sternal band. Consequently one has again to take for basis the magnitudinal relations of diagr. 7 in order to come to the conclusion, probable for an abovesaid reason as well, that the fifth rib tries to come into contact with what was formed out of the fourth rib, and not with the autochthonic sternal

formation. So, summa summarum, the sternum of the sauria consists of an autochthonic (paired) prosternum and the costal, also paired, xiphisternum, which often continues to be two xiphisterna. Of the two the autochthonic prosternum is formed first. The xiphisternum is not formed until the paired formation of the prosternum has become partially unpaired (by cranial fusion). The whole process of development of the sternum is rendered in diagr. 7; in each part of this figure the sternal formation of only one half of the body was drawn, in diagr. 7*d* half of the sternum, which is already unpaired cranially.

Now we have to consider what comparative anatomical conclusions we are brought to by the foregoing embryological facts.

According to the well known manuals on comparative anatomy by Gegenbaur, Wiedersheim and Bütschli the sternum of the tetrapode vertebrates occurs in two entirely different forms, viz. in Amphibia we find a sternum to the formation of which the very short vertebral ribs have certainly not coöperated, and in Amniota there is only a costal sternum, formed by the fusion of two so-called sternal bands, which in their turn were formed by the fusion of the ventral ends of the (vertebro-sternal) ribs. Howes [1]) designated the sternum of the amphibians as archisternum and the sternum of the amniota he called neosternum. So in amniota the archisternum has disappeared without leaving any trace and been replaced by the neosternum. (In passing I remind the reader of the episternal elements that may have been fused with the latter). So it is a generally acknowledged fact that the amphibious sternum has another genesis than that of the amniota.

Between the sternum of the amphibians and the shoulder girdle there are various relations. In Urodela and the Anura arcifera the sternum on both sides absorbs the coracoid by diarthrosis in a sulcus articularis coracoideus, just as in the Sauria. In the Anura firmisternia on the contrary the two coracoids are joined to the sternum by synarthrosis. The two epicoracoidea, too, are here fixed to each other, so that the sternum here behaves like a caudal appendix to one solid complex, which consists of the shoulder girdles on each side. Researches into the development of the sternum of the amphibians have been made by Götte and Wiedersheim. According to Götte [2]) the first formation of the sternum is originally paired. Afterwards it fuses into one whole with the point of the arch of the abdominal ribs.

[1]) Howes. „Nature". Vol. 43. N°. 1108, p. 269.

[2]) Götte. Entwickl.gesch. der Unke. Leipzig 1875.

The sternum of the Ranidae is supposed to have been formed out of the aforesaid paired first formation. At another place GÖTTE sums up his theories in the following way: the amphibians have no costal sternum. Its place is taken up by skeleton-parts of various origin, viz. 1°. by cartilage, formed in the linea alba abdominis and in the tendinous band of the m. rectus abdominis, which has to be considered as homologous with ventral ribs, and 2°. by cartilage formed in the membrane interepicoracoidea, there where the latter is inserted to the part spoken of sub 1°. the sternum of urodela and that of Bombinator (arcifera) consists of both parts. The sternum of the Ranidae (firmisternia) is supposed to have been formed only out of the part named sub 1°. the part formed in the membrane interepicoracoidea is considered by GÖTTE as belonging to the humeral zone.

The results of WIEDERSHEIM's researches may be summed up as follows. In the formation of the (paired) sternal formation neither in Anura nor in Urodela the humeral zone has any share. The whole development of the sternum takes place in, resp. between the muscles of the wall of the body. In the Amphibians, too, one has to speak of a costal sternum, for why should ventral parts of the myocommata that are becoming cartilaginous have to be considered from another morphological point of view than the ribs lying near the spinal column?

Let us sum up the facts found by GÖTTE and WIEDERSHEIM. In the first place the sternum is formed pairedly and in the second place it is formed loose from any previously existing skeleton-formation, in so far the theories agree. Only the interpretation of the facts is partly different. GÖTTE as well as WIEDERSHEIM speak of ventral ribs. WIEDERSHEIM brings back the whole of the amphibian sternum to ventral ribs. GÖTTE is in favour of a coracoidal origin for a large part of the sternum of the Urodela and that of Bombinator (arcifera), as well as for the whole sternum of the Ranidae (firmisternia), only because it grows in the membrana interepicoracoidea. But after all neither of these interpretations can explain away the fact that the first formation of the sternum of all amphibians if formed quite independently, in other words that it is authochthonic.

Consequently there is a genetic similarity between the sternum of the amphibians and the prosternum of the sauria; so they are homologous. The costal xiphisternum of the sauria is the ontogenetically as well as phylogenetically later formed sternal element. The value of this homology is not diminished by the fact that afterwards

in the sauria a varying number of ribs comes into contact with the prosternum, neither is it diminished by the fact that in the anura firmisternia the sternum becomes secundarily connected with the shoulder girdle. In this, as well as in the lately formed costal xiphisternum of the sauria one has to see adjustments to the further development of the anterior extremity as organ of locomotion and of support, a fact which is connected with the transition to landlife.

I am unable to find, aided by the study of the literature relating to this, further points of connection in crocodilia, aves and mammalia for the thesis developed in the preceding words.

RECAPITULATION.

1. The prosternum of the sauria is autochthonic and formed pairedly. A varying number of ribs becomes secundarily connected with the prosternum.

2. The xiphisternium of the sauria is costal and is also formed pairedly. To its formation cooperate those ribs that follow after those fixed to the prosternum.

3. The prosternum of the sauria is homologous with the sternum of the amphibians. Phylogenetically and ontogenetically it is older than the costal xiphisternum, developing in the sauria.

Physiology. — *"On the formation of heterogenetic antigen by combination of hapten and protein"*. By K. LANDSTEINER (Communicated by Prof. C. H. H. SPRONCK).

(Communicated at the meeting of November 26, 1921).

In former communications,[1]) which also contain references of the literature on the subject, the author came to the conclusion, that the peculiar properties of heterogenetic antigen very likely can be explained as follows.

These antigens consist of two different parts, one an alcohol soluble part (perhaps of lipoid nature) and one a protein. The alcohol soluble-part has the property of reacting specifically in vitro, but is devoid of antigenic properties (similar substances have been called by the author hapten) whereas only the entire complex (hapten + protein) acts as an antigen. Since that time, the same opinion has been expressed by TANIGUCHI. [2])

The author deemed it desirable to confirm this view by direct proof and therefore he undertook to investigate whether it would be possible to obtain an artificial antigen bycombining the hapten with a protein which as such contains no heterogenetic antigen.

It was doubtful at the onset whether this endeavour would be successful, since similar phenomena are not yet known. The experiment however gave positive results. Each of 5 groups of rabbits was injected intraperitoneally with one of the following substances.

1. Pig serum ten times diluted with 0,9 percent saline.

II. Alcoholic-extract of 15 gr. horse kidney emulsified with 100 c.c. 0,9 percent of saline.

III. As I, but heated for a ½ hour at 80° C.

IV. The extract of horse kidney emulsified with ten times diluted pig-serum.

V. As IV but heated for a ½ hour at 80° C.

The rabbits were injected with 5 c.c. of these solutions six times,

[1]) Meeting of the "K. Akad. v. Wetensch. te Amsterdam" of Februari 26, 1921. Biochem. Zeitschr. **119**. 294 (1921).

[2]) Journ. of Path. a. Bact. **24**. 253, 254. Juli 1921.

each time with an interval of one week. After the addition of $^1/_4$ percent phenol the solutions were kept in the icebox.

A week after the last injection the hemolytic action of these sera on sheep-blood was examined.

The technique used in these experiments and the indication of the results are the same as in the former communication (m. tr. = = minim trace). In the table given below the results of hemolysis are indicated.

Injection of preparation	I				II				III			IV				V			
	0	0	0	m. tr.	0	0	0	tr.	0	0	0	c.	d.	f.	m. tr.	tr.	m	f.	a. c.

The experiments will be published extensively elsewhere.

The results obtained encourage further research on the possibility of obtaining antigenic actions by combining various non antigenic substances with proteins.

Physics. — *"A moving coil galvanometer of high sensitivity"*. By Prof. F. ZERNIKE. (Communicated by Prof. H. HAGA).

(Communicated at the meeting of October 29, 1921).

Introduction. The problem to determine the conditions for which a moving coil galvanometer reaches maximum sensitivity, has been frequently discussed in the literature of the subject [1]).

The result attained is in short as follows: for any fixed period of oscillation the sensitivity varies inversely as the root of the *moment of inertia* K of the moving system.

Now the *torque* D of the suspension cannot be decreased beyond the limit determined by the smallest dimensions of suspension strip available, which limit until recently was 0,2 C.G.S. Hence K, which is proportional to D, cannot be decreased indefinitely.

In recent years several galvanometers have been constructed with much smaller torques. Even if there was no limit to the smallness of D, the sensitivity would still be restricted as K cannot be indefinitely decreased because of the presence of the *galvanometer-mirror*. Indeed it is very remarkable that in the above mentioned discussions this important detail of the instrument has hardly been taken into account. Only EINTHOVEN [2]) has laid stress upon the fact that by judging the sensitivity of galvanometers the size of the mirror ought to be taken into account. As is well known, his studies led to the construction of another type, the stringgalvanometer, hence do not answer the question which we put: *how to make a reflecting galvanometer with moving coil as sensitive as possible.*

To solve this question I will start with a given mirror. Up till now the mirror was considered to be a detrimental though a necessary addition, because it increases the moment of inertia. Indeed I found the moment of inertia of the mirror to be from 1 to 3 % of the whole system in different commercial instruments. I will invert this

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[1]) See i.a. W. JAEGER, Z f. Instrumentenk. **23,** 261 en 533 (1903).
W. P. WHITE, Phys. Rev. **19,** 305 (1904).
W. J. H. MOLL, these Proc.
Discussed at length by W. JAEGER, Elektrische Messtechnik, Leipzig 1917, pg. 204 sqq.

[2]) W. EINTHOVEN, Ann. d. Physik **12,** 1062 (1903).

and state: the coil is a detrimental though a necessary addition to the mirror, and one should take care that the total moment of inertia does not become much greater than that of the mirror.

The following formulae will show clearly what may be attained in this respect. Afterwards I shall prove that for the technical construction according to these principles one can calculate every detail of construction about in the same way as an engineer calculates a dynamo, at the same time I shall give the data of actually constructed galvanometers, as they are put on the market by KIPP and Sons Ltd., Delft.

Calculation. For the voltage sensitivity we have

$$P = \frac{Hf}{Dr} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

as condition for the limit of aperiodicity

$$\frac{H^2 f^2}{2r} = \sqrt{DK} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and for undamped oscillations

$$D = \frac{4\pi^2}{T^2} K \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Here and further on the letters have the following meaning:
P rotation in consequence of unit e.m.f. in the circuit,
H intensity of the magnetic field,
f winding surface of the coil,
D torque for unit angular displacement,
K moment of inertia of the whole system,
K_0 moment of inertia of the coil,
r resistance of the whole circuit,
r_0 resistance of the coil,
T complete period of the undamped oscillations,
$m = H/H_{min}$,
in which all quantities are to be expressed in electromagnetic C.G.S. units.

Eliminating Hf and D from (1), (2) and (3) we get:

$$P^2 = \frac{T^3}{4\pi^3 Kr} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

for the voltage sensitivity. Now it is well known that the current sensitivity of any galvanometer is proportional to $1/r$, the voltage sensitivity inversely proportional to this. Therefore the power sensitivity (Watt-sensitivity) is independent of the resistance. It is apparent from (4) that we can only increase this power sensitivity

by reducing K or by increasing T. The latter alternative, however, would soon render the galvanometer less fit; therefore I prefer to introduce at once the maximum value of T which we will allow in any special case. So we must try to find the maximum of (4) for fixed T, mirror and external resistance. Moreover I'll assume H to be given. From the result it will then be clear in which way the resulting sensitivity depends upon H. From (2) and (3) we find:

$$H^2 = \frac{4\pi}{T} \cdot \frac{Kr}{f^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Now suppose the coil to be short circuited and the mirror removed by which $Kr = K_0 r_0$, thus assuming its minimum value for the coil in use.

From (5) we then derive the minimum value of H with which the galvanometer can be made aperiodic. The importance of this minimum magnetic field, which I shall represent by H_{min}, lies in the fact that this quantity appears to be independent of the dimensions of the coil, the diameter of the wire etc. Indeed, taking only the vertical part of the windings into account, it will be easily found that:

$$\frac{K_0 r_0}{f^2} = s\sigma$$

i.e. the product of the density and the specific resistance of the metal. The horizontal part of the circuit of the coil, the insulation etc. can only *increase* the value found here and consequently H_{min}. The following relations therefore hold:

$$H_{min} = \frac{4\pi}{T} \frac{K_0 r_0}{f^2} = \frac{4\pi s\sigma}{T} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

so that H_{min} must be considered as a constant in finding the maximum sensitivity.

(5) and (6) give:

$$\frac{H^2}{H_{min}^2} = \frac{Kr}{K_0 r_0} = m^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6a)$$

in which thus m is a known number > 1. Representing K/K_0 by k, $r/r_0 = m^2/k$. Instead of (4) we can write:

$$P^2 = \frac{T^2}{4\pi^2 (K - K_0)(r - r_0)} \cdot \frac{1}{m^2} (k-1)\left(\frac{m^2}{k} - 1\right)$$

In this expression only the two last factors are variable. Their product is a maximum for $k = m$. Hence the conditions for maximum sensitivity are:

$$\frac{K}{K_0} = \frac{r}{r_0} = \frac{H}{H_{min}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and thus the maximum sensitivity:

$$P^2 = \frac{T^2}{4\pi^2 (K-K_0)(r-r_0)} \cdot \frac{(m-1)^2}{m^2}.$$

From this we derive that m should be made as large as possible. $K-K_0$ is the moment of inertia of the mirror, $r-r_0$ is the given external resistance (rather + the resistance of the flexible leads which is a known quantity in any special case). The greatest though in practice unattainable sensitivity is thus:

$$P_{max}^2 = \frac{T^2}{4\pi^2 K_{mir} r_{ext}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

whilst the ratio P/P_{max} might be called the efficiency of the galvanometer. Hence one generally finds for this efficiency:

$$\frac{P}{P_{max}} = \frac{1}{m}\sqrt{(k-1)\left(\frac{m^2}{k}-1\right)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

As m can be e.g. 10 the conditions (7) mean that one should not only — as is known from former researches — reduce the resistance of the galvanometer compared with the external resistance, *but that also the moment of inertia of the coil should be small in comparison with that of the mirror.*

Technical construction. We are going to make use of the above mentioned formulae for the further calculation of galvanometers with two different periods of 3 resp. 8 sec. For the circular mirrors which may be used we have:

Diameter	12	10	8	millimeters
Moment of inertia	0,0055	0,0026	0,0011	

for a thickness of 0,20 mm. Mirrors thinner than this are mostly insufficiently plane, besides they warp too easily in mounting.

The attainable value of H depends not only on the size of the permanent magnet which is used but also on the dimensions of the airgap. For various existing galvanometers I found for H values near 700; once I found 1100. The small coils with only few turns of wire, which are needed according to our calculations, allow to increase H considerably, provided one places an iron core inside the coil. I use for example a core of 6.8 mm. diameter and 15 mm. height, and an airgap of 1.2 mm. round it. The coil then consists of rectangular turns of wire of 8×16 mm. With a simple steel-magnet the magnetic field proved to be

$$H = 2000$$

I am going to accept these values for the following. From (6) we

derive for copper with $T = 3$ resp. 8 sec., $H_{min} = 250$ resp. 150. Taking however the horizontal pieces of the wire into account, the resistance increases $^3/_2$ times, the moment of inertia $^7/_6$ times, whence:

$$H_{min} = 330 \text{ resp. } 200$$
$$m = 6 \quad ,, \quad 10$$

Not to make the coil too thin and mechanically too weak, I'll not take in accordance with (7) $K/K_0 = m$, but $= 3$. Then according to (9) the efficiency will still be 78 resp. 80 % whereas it is 83 and 90 % in the most favourable case.

When we choose for the faster galvanometer a mirror of 8 mm., for the other a mirror of 10 mm. we get:

$$K_0 = 0{,}0005 \text{ resp. } 0{,}0013$$

and

$$D = 0{,}0070 \quad ,, \quad 0{,}0025$$

These are about the utmost values which we can use, so that smaller mirrors would hardly produce a greater sensitivity. We have namely up till now neglected *the airdamping* in our calculations. This appears to be already quite perceptible here. By further reducing the product KD which is 10.10^{-6} here, we should cause the galvanometer to be already aperiodically damped on open circuit. By using a smaller coil one could get somewhat further.

To complete the calculation the resistance must be known. As an example I choose 100 Ω for the total resistance. From (6a) we derive:

$$\frac{r}{r_0} = 12 \text{ resp. } 33 \text{ and } r_0 = 8{,}3 \text{ resp. } 3{,}0\ \Omega$$

and f from

$$\frac{K_0 r_0}{f^2} = \frac{7}{4} s\sigma = 2{,}6 \,.\, 10^{-5}$$

$f = 13$ in both cases.

As one winding has an area of 1,2 cm² we must take 11 windings with a length of wire of 51 cm. From the resistance and the length we find for the diameter of the wire 0.035 resp. 0.06 mm. The first of these two values is rather too small. We can remedy this 1st. by taking a smaller coil of 5×12 mm. e.g. thus increasing the number of turns and the length of the wire. The diameter then becomes 0.043 mm. 2nd. By not taking $k = 3$ but e.g. $k = 2$. Then we find $K_0 = 0.0011$, $r_0 = 5{,}6\ \Omega$ $f = 15.3$ diameter 0.046. A combination of both methods gives the appropriate thickness of 0.055 mm.

From (4) we derive the voltage sensitivity. For the deflection P'

per microvolt in scale divisions at a distance of 1000 divisions we find the general formula

$$P' = 0{,}57\ T^{3/4}\ K^{-1/2}\ r^{-1/2}$$

and in the two cases which have been calculated:

$$P' = 8 \quad \text{resp.} \quad 21\ {}^{mm}/_{\mu V}$$

I have actually attained sensitivities of this order, also the threefold sensitivity in the case of 10 Ω total resistance. By providing the galvanometer with a magnetic shunt, which can be moved by a screw, I was able to make H continuously variable, between the greatest value mentioned and $^1/_3$ of it. By the aid of this shunt we are able to use one single instrument for resistances from 11 to 100 Ω e.g. and to bring the instrument at once in the aperiodic condition for any resistance within these limits. For the weaker magnetic fields got in this way m will be reduced to 2 or 3. All the more reason not to take K/K_0 greater than 3.

Remains a very important matter viz. how it is possible to realize the required very small restoring torque. For a strip with rectangular section of sides a and b and length l, which is twisted, we have

$$D = \frac{G}{3l}\, ab^3$$

when b is small with respect to a. G is the torsion modulus.

One might try to cut small strips of metal foil in order to get b very small. The ordinary silver foil for instance has a thickness of 0,2 μ. Experiments showed that these strips cause a many times greater torque than that calculated from their dimensions. Apparently the beaten metal departs too much from the simple shape supposed in calculating the formula, its thickness being very uneven and its surface very rugged.

Therefore I have made silver foil of 0.4 to 0.7 μ thickness by electrolysis, for instance by precipitating the metal on zinc and afterwards dissolving the zinc in a weak acid. By means of a razor attached to a dividing engine, one is able to cut narrow strips of this foil, if necessary even of 0,02 mm. width.

For a silver strip 0.5 μ thick, 0.1 mm. wide and 10 mm. long the formula gives $D = 0.00012$. Two of these strips would thus give a torque of only one tenth of the smallest one required. Repeated experiments showed that by very careful treatment one gets with such strips a torque which is 2 or 3 times the calculated one. From the dimensions mentioned one finds further for the resistance 3 Ω

which is low enough even for galvanometers of small resistance. Of course one can also use wider strips.

It is not possible to use these thin strips as a suspension, for they show the small torsional rigidity only when unstretched. Therefore I use a quartz fibre suspension taking care that this produces 80 or 90 % of the required torque. As a consequence the elastic after-effect of the strips which is otherwise considerable, is of little or no importance. The metal strips — probably copper is in some respects to be preferred to silver — hang loosely on both sides of the quartz fibre.

Finally I will mention the fact that the very light moving systems of my galvanometers appear to be little sensitive to tremors. Surely this is at least partly due to the relatively strong air-damping which makes the vibrations about horizontal axes decay pretty quickly.

Groningen. *Physical Laboratory of the University.*

Physiology. — *"On Superficial and Internal Processes"*. By Prof. H. ZWAARDEMAKER.

(Communicated at the meeting of December 23, 1921).

In 1908 (1) I discussed, in collaboration with M. C. DEKHUYZEN, the hypothesis that animal tissues are to be conceived as a system of co-existent phases. Phases have been defined there as a condition of the substance homogeneous at a given moment. In life the stationary current of metabolism with its periodic fluctuations passes through such a system. Once every 24 hours the condition approaches nearly the equilibrium during profound sleep.

In such a system it is especially the boundaries of the phases that are important, for directly when action sets in chemical processes manifest themselves near the boundaries. This may be expected a priori and is proved a posteriori by the great importance physiology has to attach to surface-tension and to membrane-potentials.

Such important phase-boundaries in a tissue are, generally speaking:

a. the cell-surfaces,

b. the contours of the nucleus,

c. the contours of the mitochondriae,

d. the mantle-, and contact-planes of double-refracting pieces of fibrils,

e. the contact-planes of the neurobions of Cajal.

Also in the phases themselves processes occur, as physiology usually assumes. To these processes belong i. a. stationary metabolism, just now alluded to, heat-production, growth.

A general physiology of the phases has to distinguish between superficial and internal processes.

I have had many occasions to occupy myself with the first category, since *all* processes in which animal radio-activity comes into play, belong to the sub-superficial processes, while the second category i. e. the internal processes comprises *all* processes to which BAAS BECKING (3) has lately applied the theory of PERRIN in such a signal way.

This should not tempt us to generalize prematurely and to advance the hypothesis that in nature all superficial processes have corpuscular radiations for catalyzator, and all internal processes electromagnetic vibrations, and that beyond these radiations there are no other biological catalyzators. We do not feel justified in deducing

such bold conclusions, since we are completely in the dark about the oligodynamic actions of the elements Ferrum, Calcium, Magnesium. Positive facts, however, prompt us to assume that the stimulating effect of the radiations, already discovered, play a prominent part.

Most of all our knowledge of the cell-surface as a phase-boundary has increased. A lipoid-layer is generally assumed there. Some hold this to be an illustration of GIBBS's theorem, according to which substances always aggregate on a boundary-layer, which lower the surface-tension in situ. This deduction is not admissible, for nothing is known about the power of lipoid to lower the surface-tension protoplasm—tissuefluid. The data concerning the boundary-layer water—air are not immediately applicable to other boundary-layers. The determinations made in my laboratory concerning the boundary-layer oil—water have shown great differences with the boundaries water—air. Therefore, the hypothesis of a lipoid boundary-layer can only be admitted as a working-hypothesis not as a deduction. If, furthermore, this layer is assumed to be one single layer of molecules, after LANGMUIR (4), then with a small amount of lipoid some spots will remain intact. In this way the so-called mosaic-hypothesis has been explained physically.

For a long time already charges have been assigned to cell-surfaces. J. LOEB and BEUTNER made valuable researches in this direction and some time ago J. LOEB and his co-workers reduced all to equilibria of DONNAN. Quite a different theory was set forth by T. P. FEENSTRA (6) in my laboratory. He abandons the membrane-conception, and imagines the elements Na, K, Ca aggregated in the form of fixed, non-ionised compounds, on a large number of points of the cell-surfaces. In that case a solution-potential must be generated on these spots according to NERNST's theory. From these points some atoms will pass into the surrounding tissue-fluid in the form of cations. Consequently the loss of positive charge will originate on those spots a negative potential, which will increase until the escaping cations are in equilibrium with those of the same sort which are already present in the tissue-liquid. The various metal points can be of the same potential only when the cations in the tissue-fluid are present in a certain ratio. From experimental data he calculated this ratio for the three elements $\frac{Na+K}{Ca}$ and found that it agreed with the long known ratios of the balancing ions in sea-water and in the solutions of S. RINGER. In this striking quantitative concordance he sees an affirmation of his theory.

Suppose in the interstices between the metal-spots lecithin and cholesterin to be aggregated in a layer of LANGMUIR, then it will be easy to imagine the quantity $\frac{\text{lecithin}}{\text{cholesterin}}$ present to such an amount that here again the same potential is reached, as has been forced upon the metal spots by the tissue-fluid. Only then will the potential of the cell-surface be considered equal everywhere, and will the condition for electric rest be satisfied.

Now from the cell-surfaces, thus equipped, a number of actions proceed which depend on corpuscular radiation. To these belong:

a. A number of automaticities such as 1° the automaticity of the heart of cold-blooded animals (7) (frog, toad, eel); 2° the automaticity of the heart of a warm-blooded animal (8) (rabbit); 3° the automaticity of the muscle fibres of the gut (rabbit, cat, mouse) (9); 4° the automaticity of the esophagus (10); 5° the automaticity of the muscular fibres of the uterus (11);

b. The synapsis-effects between vagus and cardiac muscle (12), between vasomotors and muscular coats of the arteries (13);

c. The permeability of the capillary endothelia for water (14);

d. The permeability of the glomerulusepithelia for glycose (15);

e. The synapsis-effect between motory nerve and voluntary muscle (16) (on this occasion the *distal* contact-plane appeared to be sensitive to radio-activity).

All these actions are subject to the law of aequiradio-active substitution (17) and that of radio-physiological antagonism (18).

When, in typical cases, these actions are brought about from some distance by free radiation (19), it appears that they possess a rather long latent period and a rather long after-effect, but when called forth materially by radio-active elements, the process is rapid and there is no time for penetrating of the ions or micella (20) into the interior of the cell. In that case there can only be question of a superficial process.

The dosages of the added radio-elements, required for the action, may be modified by sensibilizators (21), some of them working through modifications of the adsorptions. This holds for fluorescein and eosin, as well as for adrenalin and cholin. The first two substances can supersede each other in a schematical experiment; talcum venetum figures as an absorbent. Likewise eosin can supersede fluorescein. The reverse, however, is not possible (22). Also adrenalin and cholin are mutually antogonistic (23). In the heart-cells these substances are related just as in the schema: the supersession eosin-fluorescein is biologically one-sided, the supersession

adrenalin-cholin is biologically reciprocal. The sensibilization through adrenalin and cholin with its peculiar properties in respect of both kinds of corpuscular radiators has thus far been observed, besides for the heart, also for the synapsis between vasomotors and vascular muscle-wall. Another part of the substances, which in oligo-dynamic addition can bring about a sensibilization of some duration, have the character of cytolisins: (in a higher concentration they cause hemolysis). Most often cytolysis is attributed to dilution of the boundary-layer. When we endorse this hypothesis, also this form of sensibilization is acknowledged as a form of superficial effect (24). FEENSTRA has demonstrated that for some of the radio-active elements, which can replace potassium, the proportion relative to Na and Ca which is required to satisfy the balancing-equilibrium, can be calculated. This proportion appears to fall within the concentration-latitude, in which the replacement may be applied. For other substitutes calculation is impossible as they are colloidal in the modified Ringer-solution. In this case, however, there can be no question about interior action: their activity can be interpreted only by adsorption to the cell-surface. So long as they are in suspension in the circulating fluid they can evidently not exert any raying-effect worth mentioning.

Beneath the cell-surface, to which we suppose the radio-active elements to be attached as metal-points in fixed protein-composition, or to be adsorbed in colloidal or atomic composition, the electric phenomena are at work. This appears from the fact that the electrocardiogram of a uranium-, thorium-, or ionium-heart, is not distinguishable from that of a potassium-, resp. rubidium-heart. Anyhow not in the beginning. Also the electrocardiogram of a heart, which pulsates with S. RINGER's solution without calcium, and whose cells must therefore be imagined devoid of calcium metal-points, remains unchanged in the beginning. Since the hypothetical lipoid-layer can never be continuous, and therefore can never represent a perfect dielectricum, the old theory of ENGELMANN is still intact, according to which the transmission of the stimulation into the mass of the heart-muscle is brought about by the transmission of the current from cell to cell.

In this paper there is no opportunity for a further exposition of the surface-hypothesis for other cases than those round the heart muscle-cells. It cannot be expected that all cell-surfaces have their own electric charges, nor need the hypothetical catalysators occur on all contact-planes.

REFERENCES.

1. H. ZWAARDEMAKER en M. C. DEKHUYZEN, Ruhende tierische Zellen und Gewebe; ein verwickeltes System koexistierender Phasen. Int. med. Kongr. Budapest 1908.

2. H. ZWAARDEMAKER, Erg. der Physiol. 1906, Bd. 5, S. 125.

3. L. B. BECKING, Radiation and vital Phenomena. Thesis, Utrecht 1921.

4. J. LANGMUIR, Amer. Chem. Soc. Vol. 38 & 39, 1916, 1917.

5. LOEB and BEUTNER, Bioch. Ztschr. Bd. 59, S. 195. 1914.

6. T. P. FEENSTRA, Ionenbalanceering Diss. Utrecht 1921. Onderzoekingen Physiol. Lab. 6e Reeks. Vol. II, s. 1.

7. H. ZWAARDEMAKER, Kon. Akad. v. Wetensch., Amsterdam, 30 Sept. 1916.

8. E. H. JANNINK en T. P. FEENSTRA, Ned. Tijdschr. v. Gen., 1920, II, N°. 15.

9. E. H. JANNINK, De invloed van het kalium op de beweging van den darm. Diss., Utrecht 1921.

10. C. E. BENJAMINS, Physiologendag 1920. (Not yet published in extenso).

11. To be published.

12. H. ZWAARDEMAKER en J. W. LELY, Arch. néerl. de Physiol. 1917. II, p. 745.

13. K. F. A. HALBERTSMA, Physiologendag 23 Dec. 1921.

14. I. GUNZBURG, Arch. néerl. de Physiol. 1917, T. II, p. 404. (Compare also Ned. Tijdschr. v. Gen. 1917, T. II, N°. 14).

15. H. J. HAMBURGER en R. BRINKMAN, Kon. Akad. v. Wetensch., Deel 26, blz. 952.

16. I. GUNZBURG, Nat. en Geneesk. Congres, 's-Hage 1917.

17. H. ZWAARDEMAKER, Am. Journ. of Physiol. Vol. 45, p. 147, 1918. (Compare Kon. Akad. v. Wetensch., Amsterdam, 20 Sept. 1916).

18. H. ZWAARDEMAKER, Kon. Akad. v. Wetensch., 24 Febr. 1917, 31 Maart 1917, Pflüger's Archiv., Bd. 169, S. 122, 1917.

19. H. ZWAARDEMAKER, C. E. BENJAMINS en T. P. FEENSTRA, Ned. Tijdschr. v. Geneesk., 1917, II, blz. 1923.

20. H. ZWAARDEMAKER, Pflüger's Arch., Bd. 173, S. 51, 1918.

21. H. ZWAARDEMAKER, Kon. Akad. v. Wetensch., Amsterdam, 29 Sept. 1917.

22. A. M. STREEF, Onderz. Physiol. Lab. (5), Deel XIX. (Compare also Pflüger's Arch., Bd. 173, S. 60).

23. J. B. ZWAARDEMAKER, Physiologendag, Amsterdam, 22 Dec. 1921.

24. The experiments on sensibilization through cholin and adrenalin will be published in extenso later on by Mr. NYHUIS. For sensibilization in connection with the law of aequiradio-activity, see H. ZWAARDEMAKER Arch. Intern. de Physiol., T. 18, p. 282, 1921.

Botany. — *On Hypotriploid Dwarf-hyacinths derived from Triploid Dutch Varieties through Somatic Variation.*" By W. E. DE MOL. (Communicated by Prof. J. C. SCHOUTE).

(Communicated at the meeting of December 23, 1921).

I. *Introduction.*

Among the varieties of Dutch hyacinths, which in my cytologic investigation I found to be heteroploid, there are a couple which produced a comparatively large number of budvariations: varying in *form* as well as in *colour*. They are the single triploid varieties *Grand Maître* (colour: pure mauve), and *King of the Blues* (colour: ± like Prussian-blue).

In my publication: "*De l'existence de variétés hétéroploïdes de l'Hyacinthus orientalis L. dans les cultures hollandaises*" I made mention of a large, elongated form of *Grand Maître*, termed by me *Grand Maître giganteus* whose morphological aspect of chromosomes, so far as could be made out, is exactly the same as that of the parental variety. Furthermore, I pointed out that small bulbs also agreed in number, shape and size of the chromosomes, so that this growth-retardation had to be ascribed to external circumstances.

As to the variety *King of the Blues* I reported only parenthetically, that morphological change was observed with a vegetative increase. In the autumn of 1920 and in 1921 I was in a position to establish, without sacrificing all my anomalous plants, that what I had wrongly presumed regarding *Grand Maître*, proved to be a fact, viz. that through vegetative increase dwarf-plants could arise independently of each other, characterised by a smaller number of chromosomes in the root-cells, which number was still the same after years of vegetative propagation.

Many conjectures may be made as to the cause why it is just the *Grand Maître* and the *King of the Blues* which are characterised by marked budvariations: 1. that these varieties are heterozygous triploid; 2. that they are grown in large numbers; 3°. the mentioned facts combined.

II. *In which respect do the dwarf-plants differ in their outward aspect from the mother-variety?*

It was quite a matter of chance that I came into possession of my dwarf-hyacinths: When inspecting in flowering-time the deep-blue spikes of *King of the Blues* one will sometimes detect plants whose inflorescence is of a dark pink colour entirely or partially i.e. in a larger or smaller sector. This budvariation occurs repeatedly. Commercially it is termed *Queen of the Pinks*. The number of chromosomes agrees with that of *King of the Blues*.

Now I observed, in different places, in two separate batches of *King of the Blues*, quite apart from each other, somatic varieties, which were conspicuous for a carmin-red colour of the flower, i.e. a darker colour than that of *Queen of the Pinks*. I consequently cultivated them. As they were growing among small, young plants of *King of the Blues* the small dimensions did not strike us at once. Only after some years did these differences manifest themselves. As to *forms* they resemble exactly *King of the Blues* and *Queen of the Pinks*. As to *dimensions*, however, they are much smaller. The flowerspikes, the separate flowers and their parts, the position of the flowers upon the peduncle, the narrow, stiff stalk-leaves, so characteristic of these varieties, in all the *shape* is the same, the *size* is different.

The flowering-times agree. The foliage withers simultaneously. How great the quantitative differences of the fullgrown bulbs are, may be best gathered from the following table. In succession the circumference, the weight and the volume of the bulbs of *King of the Blues*, *Queen of the Pinks*, *dwarf n°*. 1 and *dwarf n°*. 2, are determined.

King of the Blues	23 c.M.	149 G.	150 c.M.^{3}
Queen of the Pinks	23,5 „	161,5 „	160 „
Dwarf n°. 1.	12 „	28,5 „	26 „
Dwarf n°. 2, 1st bulb	11 „	20 „	20 „
id , 2nd bulb	10,5 „	19,5 „	20 „
id , 3rd bulb	10 „	20 „	20 „

But besides these quantitative differences the difference in the *rate of vegetative propagation* is also remarkable. It is safe to say that *King of the Blues* and *Queen of the Pinks* propagate vegetatively ten times quicker than the dwarfs, whether they multiply

in a natural way, or whether they are urged to rapid production of bulbils through crucial incisions in the discus.

I know of no variety that grows so slowly. That is why of the 2nd dwarf I possess only 20 bulbs after eleven years' careful cultivation. The 1st dwarf is still slower in its growth.

The roots of the dwarfplants are thinner than those of the parent-variety. Never did I come across hyacinth-species with roots as thin as those of *dwarf n°*. 2.

III. *Cytological inquiry into the two dwarf-shapes.*

In the autumn of 1920 and in 1921 I have fixated repeatedly the root-tops of 1 bulb of *dwarf n°*. 1, in 1921 the root-tops of 3 bulbs of *dwarf* n°. 2. The fixation was performed with Flemming's solution and glacial acetic vinegar. My wife has performed the rest of the technical work.

The investigation of the root-sections showed very distinctly that the *somatic nuclei of dwarf n°*. 1 *consisted of* 18 *chromosomes and*

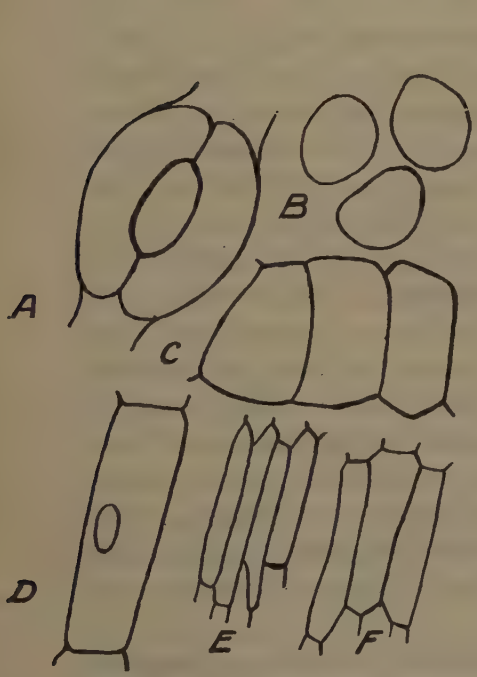

Fig. I. *Dwarf n°. 1*: *a*, stoma (Oc. 4, Obj. *D*, Zeiss.); *b*, pollengrains (Oc. 2, Obj. *D*); *c*, subepidermal cells of the tepals (Oc. 2, Obj. *D*); *d*, epidermal cell of the tepals (Oc. 2, Obj. *D*); *e*, cells of the outer bulbscales Oc. 2, (Obj. *D*). *Dwarf n° 2*: *f*, cells of the outer bulbscales (Oc. 2, Obj. *D*.)

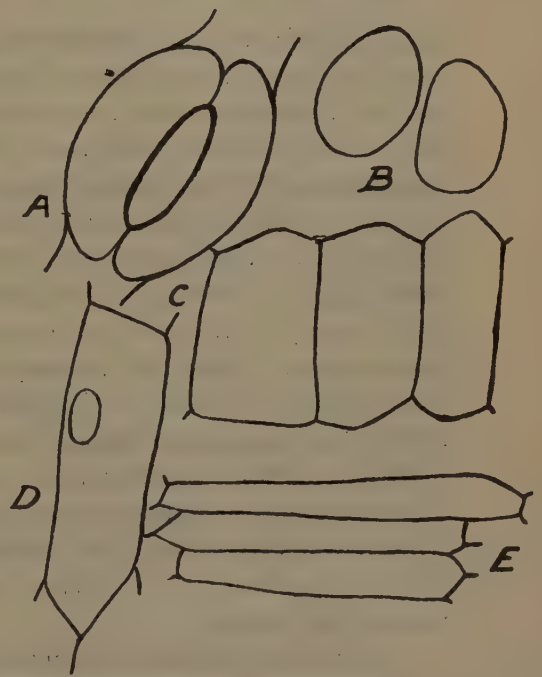

Fig. II. *Queen of the Pinks*: *a*, stoma; *b*, pollengrains; *c*, subepidermal cells of the tepals; *d*, epidermal cell of the tepals; *e*, cells of the outer bulbscales. Magnification as in fig. I.

those of dwarf n°. 2 of 21 chromosomes, consequently of resp. 6 and 3 chromosomes less than the nuclei of *King of the Blues* and *Queen of the Pinks*. Likewise the number of nucleoli in the nuclei of the dwarfs is smaller. The dimensions of the chromosomes and of the nucleoli have not altered. The cells of pleroma, periblema and dermatogen of the roots are strikingly small, notably those of *dwarf n°. 1*, which renders the determination of the number of chromosomes extremely difficult.

From the above we may fairly conclude that the volume of these cells has relatively decreased much more than that of the nuclei, so that the difference between the magnitude of the cell and the nucleus of *King of the Blues* and of *dwarf n°. 1* is not quite the same as R. HERTWIG's nucleus-plasma relation (1903).

Stomata, pollengrains, cells of the leaf, the tepal and the bulb-scale have been measured and sketched by me. Every time the difference in the magnitude of the cells of the dwarfs and of *King of the Blues* and *Queen of the Pinks* revealed itself distinctly.

This leads to two remarkable conclusions: The first is that it must be owing to the smaller magnitude of the subepidermal tepal-cells of the dwarfs that the dissolved anthocyanin is of a carmin-red tint instead of dark pink, as in *Queen of the Pinks*. The anthocyanic pigment will be of similar chemical composition, but in the tepals of the dwarfs it will be present in a higher concentration. The difference of tint is also very well distinguishable microscopically. The higher concentration of the anthocyanin in the dwarfs shows itself clearly in the cells of the outer bulb-scales, immediately after digging-time. Then the anthocyanin occurs in them in markedly plasmolyzed vacuoles or in greater and smaller conglomerates of crystals, as MOLISCH (1905) pictures them of *Pelargonium*. It is evident, therefore, that in their smaller cells the dwarfs possess an amount of anthocyanin as great as that in the larger cells of *Queen of the Pinks*, so that the hereditary factors, which determine the chromogen and its oxydase for the two dwarfs and *Queen of the Pink*, are presumably identic.

The second conclusion concerns the fertility of the pollengrains of *King of the Blues*, *Queen of the Pinks* and of the dwarfs. For several years in succession I found that *King of the Blues* and *Queen of the Pinks* have a percentage of sterile pollengrains, which, under seemingly normal circumstances, varies between 30 and 50. Under similar conditions the percentage of sterile pollen-grains of *dwarf* N°. 1 is much smaller. It does not exceed 10 %.

Researches into the dying-off of pollen-grains after the reduction-division in the hyacinth have taught me, that this is most likely due to the fact that the protoplasm present, is too small for such large pollen-grains as *King of the Blues* and *Queen of the Pinks* possess. Now it seems to me that the greater fertility of *dwarf* n°. 1 is directly related to the quantum of protoplasm at its disposal, which will probably be sufficient for its smaller pollengrains. I have not yet been in a position to examine the fertility of the 2nd dwarf.

In the literature I know of no instances of dwarf-forms among the higher plants, arisen through somatic variation, that can be differentiated cytologically. It is an unshakable fact, that these dwarfs descend from *King of the Blues*, but particulars about the true cause of these dwarf-plants are still lacking. First of all the injuries evolved by crucial incisions might be made answerable for it. The interpretation of the observed phenomena would then accord with the opinion of SAKAMURA (1920), that no autoregulative reduction of the chromosomes in somatic hyperchromosomal cells takes place. Secondly the cause might be looked for in the fact, that the mother-plant is triploid; this may induce something like a regulating process, which, when telling to full advantage, would result in the diploid condition.

In that case the phenomena, appearing with the nucleus-division in the soma of the plant, would run parallel with those occurring with the heterotypical division of the pollen parent-cells. I refer to my description of the reduction-division of the 27-chromosomal variety *L'Innocence*.

When assuming moreover with WINKLER (1916 page 522), that according as the number of chromosomes exceeds more and more the diploid number, the chances of disturbances in the process of the somatic nucleus-division will increase, *then there is besides the reason mentioned in my publication "Nieuwe banen, etc.", still another argument that justifies my warning not to go on exterminating the old, valuable diploid Dutch hyacinth-varieties.*

It seems probable to me that dwarf-hyacinths occur not unfrequently through somatic variation of heteroploid species, but that they are generally not observed, or are thrown away as being unfit for cultivation. I know for certain that from the heteroploid pink-coloured variety *Moreno* a darker dwarf-form is derived, which however got lost before I was able to examine it. About six years ago a variegated plant was derived from the heteroploid variety *Queen of the Blues*. The bulb of this plant (which is in my possession)

remains of very small dimensions in spite of the best culture-conditions.

Throughout this investigation my interest in the cytological research into somatic varieties has increased especially in connection with the details discovered in the two dwarf-plants, and in connection with the question of the influence of the chromosomes.

Perhaps what WINKLER reported about his further experiments with Solanum (cf. SIRKS (1921)) in the 1st. meeting of the "*Deutsche Gesellschaft für Vererbungswissenschaft*", very nearly approaches the phenomena observed by me. It should be borne in mind, however, that WINKLER observed the reduction of the number of chromosomes after propagation *through seeds*, whereas I established this reduction during the *vegetative* propagation. The plants of WINKLER are *homozygous*. My plants are *heterozygous*.

REFERENCES.

R. HERTWIG, 1903, Ueber Korrelation von Zell- und Kerngrösse und ihre Bedeutung für die geschlechtliche Differenzierung und die Teilung der Zelle, Biol. Centralbl., Bd. XXIII, S. 49.

W. E. DE MOL, 1921, De l'existence de variétés hétéroploïdes de *l'Hyacinthus orientalis L.* dans les cultures hollandaises, Arch. Néerl. des Sciences exactes et naturelles, série III B, tome IV, p. 18.

W. E. DE MOL, 1920, Nieuwe banen voor het winnen van waardevolle varieteiten van bolgewassen. Overgedrukt uit het Weekblad voor Bloembollencultuur, nos. 37, 41 en 44—48 van den 31en jaargang.

H. MOLISCH, 1905, Ueber amorphes und kristallisiertes Anthokyan. Bot. Ztg., Bd. LXIII, S. 145.

TETSU SAKAMURA, 1920, Experimentelle Studien über die Zell- und Kernteilung mit besonderer Rücksicht auf Form, Grösze und Zahl der Chromosomen, Journ. of the Coll. of Science, Imp. Univ. of Tokyo, Vol. XXXIX, Art. 11.

M. J. SIRKS, 1921, „Deutsche Gesellschaft für Vererbungswissenschaft", Genetica, Deel III, Afl. 3 en 4, blz. 412.

HANS WINKLER, 1916, Ueber die experimentelle Erzeugung von Pflanzen mit abweichenden Chromosomenzahlen, Zeitschr. für Botanik, Bd. 8, S. 417.

HANS WINKLER, 1921, Ueber die Entstehung von genotypischer Verschiedenheit innerhalb einer reinen Linie, Deutsche Gesellschaft für Vererbungswissenschaft. Bericht über die Gründung und die erste Jahresversammlung, S. 16. Leipzig, Borntraeger, 1921.

From the Laboratory for the Physiology of Plants of Prof. ED. VERSCHAFFELT. *Hortus Botanicus, Amsterdam.*

Zoology. — "*On Budding and coalescence of Buds in Fungia fungites and Fungia actiniformis.*" By H. BOSCHMA. (Communicated by Prof. C. PH. SLUITER).

(Communicated at the meeting of November 26, 1921).

Budding in adult corals of the genus *Fungia* was first described by SEMPER [1]) in a specimen which had most likely been thrust upside down fortuitously. In their further growth the septa had bent round the border and in various places new mouths had arisen on the original underside, round which the later formed septa were arranged more or less radially.

From this SEMPER concludes: "Es geht daraus hervor, dass alle diese Polypen ohne Ausnahme die Fähigkeit besitzen, an ganz beliebigen Stellen ihres Körpers neue Individuen zu erzeugen, wenn durch irgend eine Ursache — physiologisch-chemische oder rein mechanische — ein besonderer Anstoss zum Hervortreiben plastischer Massen gegeben ist." [2])

Judging from the figure (Taf. XXI, fig. 3) some at least of these buds are to be considered as calicular buds as they are lying entirely on the curled-up border. Of course, it is within the bounds of probability that they are lateral buds generated through the broadening of a number of spines into septa, a process easily to be watched in the ordinary lateral budding.

Lateral budding (at the underside of the disc, which side corresponds to the lateral side of other corals) is of rather frequent occurrence in *Fungia fungites* (L.). [3]) This mode of asexual reproduction has been described at length by DÖDERLEIN. [4]) However, he does not assign a cause for this budding, probably because DÖDERLEIN did not work with fresh material. In a large number of *Fungia fungites*,

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[1]) C. SEMPER. Ueber Generationswechsel bei Steinkorallen und das M. Edwards'sche Wachsthumsgesetz der Polypen. Zeitschr. f. wiss. Zool. Bd. XXII. 1872.

[2]) l. c. pag. 275.

[3]) SEMPER reports also a case of budding at the underside of a specimen of *Fungia Linnaei* Val. (= *F. repanda* Dana) (l.c. pag. 275, note 1.)

[4]) L. DÖDERLEIN. Die Korallengattung *Fungia*. Abh. der Senckenb. naturf. Ges. Bd. XXVII, 1902.

which I collected near the island of Edam in the Bay of Batavia, we could detect at the underside buds in various stages of development; they are seen nearly always in the immediate vicinity of part of the parent-coral which is overgrown profusely with algae. It frequently happens that a bud appears at the underside, just beneath that portion of the upperside that is grown over with algae. When a *Fungia fungites* is partly attacked by sea-weeds, the latter impart a stimulus to the adjacent tissue, which consequently displays a more energetic growth-activity. This greater activity is also manifested in an increased Skeleton-production, resulting in the formation at the underside of larger spines, which are sometimes branched out, or even in the formation of buds; at the upper side this intensified growth engenders new septa, which are often of an irregular shape, while in some cases buds are formed.

DÖDERLEIN already suspected that calicular budding occurred also in *Fungia fungites* but he could not prove it. In a few specimens of this species found near Edam, rather distinct buds were formed at the upper-side of the disc; one of these specimens was very conspicuous. In this *Fungia* (Fig. 1) part of the disc is grown over with sea weeds of various kinds and with case-worms, which causes the tissues of the polype to be destroyed at this spot.

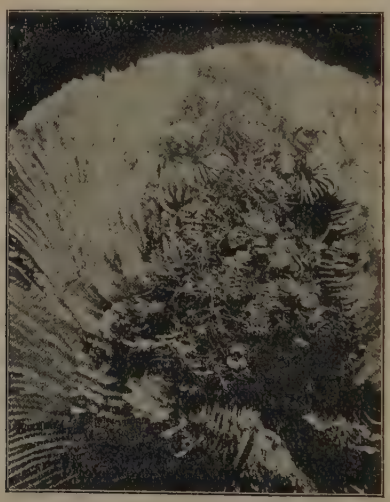

Fig. 1. *Fungia fungites.* Upper-side. Calicular budding around a part grown over with seaweeds and other organisms. $^3/_5$ nat. size.

An abundant growth of algae is also observed over the mouth. Greater growth-activity is shown round the attached part which generates new septa everywhere at the borders of the destroyed

tissue. At the intact side mouths have originated by the side of these new septa, so that ultimately instead of the old lost mouth the *Fungia* possessed about twenty new, small orifices around the algae-covered zone. A few of these mouths are environed by new septa (see the upperside in the figure); these buds therefore present a more regular aspect than the others, in which the mouth is at one side surrounded by a semicircle of new, young septa, which unite at the other side with the unattacked septa of the parent [1]).

Also at the border of the disc of *Fungia fungites* buds may arise by constricting off part of the septa of the parent-polyp and by the formation of a new mouth. This, then, is also a case of calicular budding. In its initial stage it is seen in the specimen which is represented partially in fig. 2.

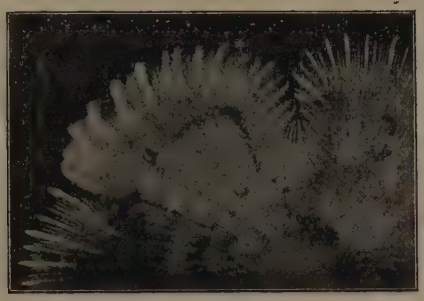

Fig. 2. *Fungia fungites*. Underside. Part of the border is grown out towards the underside. 3/5 nat. size.

At its underside a groove is noticeable at some distance from the border. This may be a scar of an old wound and the border may have been renewed at this spot through regeneration. At the periphery part of the border has curved downwards, the border has, so to speak, doubled up here and parts of the septa are lying at the underside of the disc. For the rest this *Fungia* looks quite normal. Now when this curved portion is cut off, we obtain a bud here also, a calicular one at the underside of the parent-coral.

This budding is seen further developed in another specimen (fig. 3).

[1]) In this specimen the algae-parasitism has proceeded right across the disc as far as the underside (in *F. fungites* the disc is provided with pores), which also here has given rise to a number of lateral buds, although only a small portion of the tissue of the underside has been destroyed. These buds are rather large (the largest is 32 × 25 mm.), the oldest have already a broadened border, as may be distinctly seen, so that they are attached to the underside by a stem.

Here the border has bent down at one place, just as in the preceding case, but here the septa do not merge into those of the

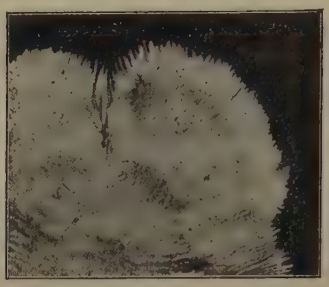

Fig. 3. *Fungia fungites.* Underside. Calicular budding at the border. $^3/_5$ nat. size.

mother-coral, the bud having become more or less independent. A mouth has already been formed, completely encircled by septa, so that a separation of the bud from the parent has already been established. The part belonging to the bud is already easy to distinguish from that belonging to the parental coral, which was not practicable in the case previously described. Now when this separation becomes more evident, the whole aspect is that of a bud at the underside. Such a bud would then be considered as a lateral bud, although ab origine it was a calicular one.

In this way may have arisen some of the buds of SEMPER's specimen alluded to above, since in the figure the septa of the old coral touch those of the buds.

The buds above-described are all either devoid of a stem, or provided with a short stem. In one specimen I found at the underside a bud with a longer stem, such as are generally found at an anthocormus. The upperside of the disc of the *Fungia* under discussion is quite normal, while the underside differs from that of normal specimens (fig. 4). The central part is rather sharply isolated from the border, while part of it is defunct. We are impressed with the idea that when this coral had a diameter of about 3 cM., the tissues of the one moiety died off for some reason or other, while from the other half a regeneration was started, which caused the coral to ultimately grow up to 8 or 9 cm. and after this to present a normal aspect. But the defunct portion maintained itself and leans on the living portion like a scale. Attached to this defunct portion we observe a stemmed young bud 6 mm. in diameter, while the stem itself is 8 mm. in length. The extremity has not yet broadened into a disc.

This case slightly reminds us of the aspect (in *F. agariciformis* = *F. fungites*) of a number of young stemmed Fungiae on the

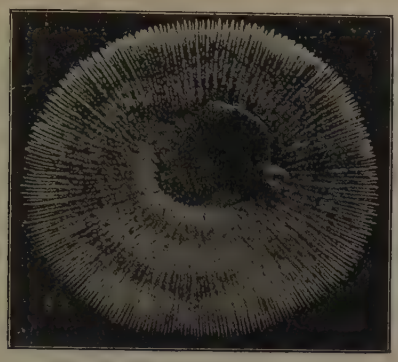

Fig. 4. *Fungia fungites.* Underside. Stemmed bud at a defunct part in the middle. ½ nat. size.

defunct disc of a coral of the same species, as described by STUTCHBURY. [1]) The same aspect was presented by some of the Fungiae I found near the island of Edam. At the underside of one of them residues of living tissue were distinguishable, but all the softer parts of these corals were vanished and here and there seaweeds and *serpulids* had settled. At the border of all the specimens there are a large number of buds, while in a few of them buds have also been formed near the central part of the underside. Of the latter the stem has a uniform breadth everywhere, in contradistinction to many at the border, whose stem has broadened at the upperside into a disc-shaped young Fungia. The stem of many buds adhering to the border of the underside, has bent round, so that the disc of the young Fungia is seen at the upperside of the border of the old coral. Some of these young corals are overgrown with algae, most of them are fully alive and look quite normal.

STUTCHBURY [2]) considers the occurrence of young corals on a defunct disc of the same species to be something accidental. ("I consider the cases in which young Fungiae are found fixed to the underside of others of the same species, to arise from the accidental attachment of the young polype"), whereas SEMPER holds that these young corals have arisen in situ through budding of the coral; the

[1]) S. STUTCHBURY, An Account of the Mode of Growth of Young Corals of the Genus Fungia. Trans. Linn. Soc. London, Vol. XVI, 1833.

[2]) l. c. p. 497.

genesis of the budding has then to be looked for in an alteration of the natural position.

MOSELEY [1]) in studying *Fungia fungites* [2]), found a portion of a very large defunct Fungia quite covered with numerous young colonies of various ages. According to MOSELEY they arose from larvae which had attached themselves to the expired Fungiae.

SAVILLE KENT also describes these Fungiae with young stemmed specimens (in *F. discus* = *F. fungites*) and gives us a picture of one of these with 13 stemmed young corals at the upper-side (Plate XXIV, fig. 1).

Although he does not dictate either the one or the other conception, he deems it most probable that we have to do here with a case of budding: ("It is a moot point whether this luxuriant colony of Nursestocks arose fortuitously from different sources, or in a single embryonic swarm from some more distant corallum, or whether they may not represent the product of the expiring vital energy of the defunct adult corallum to which they are united. The latter interpretation appears to be the most reasonable" [3]).

According to DÖDERLEIN the occurrence of colonies of young Fungiae on defunct corals of the same species has nothing to do with budding; these young corals he believes to have arisen from extraneous larvae.

The specimens I collected near Edam all lay in a normal position, orifice upwards. They exhibit some peculiarities which point to true budding. The specimens alluded to (cf. fig. 5) deviate from those described by STUTCHBURY, MOSELEY and SAVILLE KENT, in that young polypes occur only at the border of the upperside of the disc and not in the centre. Nor do these buds attain the size of those of the Fungia illustrated by SAVILLE KENT.

Each specimen is provided with a great number of these young corals, one of them with as many as 73 buds. We deem it highly probable that this is a case of true budding. The following arguments lend support to our view:

1. The rest of the parent-coral is quite defunct or nearly so. Budding is considerably promoted by algae-parasitism, as has been pointed out above. Here it arose most likely as the final manifestation of vitality of a doomed individual.

2. These stemmed buds are found only at the border, and not

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[1]) H. N. MOSELEY, Notes by a Naturalist on the Challenger. London, 1872.

[2]) Determined by QUELCH as *Fungia discus* (= *F. fungites*). (J. J. QUELCH, Report on the Reef-Corals. Challenger Exp. Zoology, Vol. XVI, 1886).

[3]) W. SAVILLE KENT, The Great Barrier Reef of Australia. London 1893, p. 38.

farther on, at the upperside. If larvae had given origin to these young corals, they would not have been disposed so regularly in one row along the border.

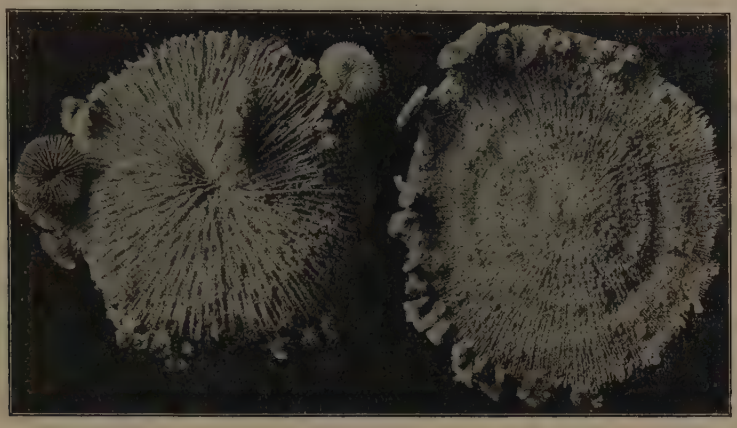

Fig. 5. *Fungia fungites.* The left specimen seen from above, the right one from below. Budding at the border of individuals almost entirely overgrown by parasites. 3/8 nat. size.

3. Also at the underside some buds are noticeable, rather removed from the border. When a Fungia is attacked by algae, the tissue of the underside keeps intact longer than any other part, because the weeds have to force their way through the disc in order to attack it also.

This is why the tissue at the upperside may be entirely destroyed by algae, while rests of soft parts may still exist at the underside, which may induce the growth of buds.

These buds do not develop so well as those at the border, their extremity is not broadened into a disc, which is probably due to the absence of light. The development of these very buds goes against the hypothesis that they should have originated from larvae which had attached themselves here. The old Fungia lies flat at the bottom, even the borders are still covered with sediment [1]). Owing to this the underside is isolated from the environment, so that no larvae can settle there, putting aside the very unpropitious position occupied by these buds with respect to the light.

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[1]) A living Fungia continually removes the sediment that falls on the upperside, by enclosing it in a layer of mucus, which is removed from the centre onward over the border.

4. I found these stemmed young Fungiae only on the disc of the defunct Fungia and not on the defunct coralfragments in the neighbourhood. If these young polypes had arisen from a swarm of larvae, which had settled down at the same time or at different epochs, a few would no doubt have found a base of attachment in the neighbourhood.

Colonies of fixed young Fungiae (Anthocormus) are especially known of *Fungia fungites*. Of *Fungia actiniformis* Q. et G. fixed young corals have been described by STUDER [1]). Afterwards no more mention is made of anthocormus-formation in this species. Still, it seems to occur occasionally; as I found on the reef round Edam about 24 young colonies of Fungia actiniformis. The number of buds at every anthocormus differs largely. One of these specimens possesses 48 buds or stems from which the young coral has detached itself. Upon a number of these stems, a new bud is already developing. The largest young Fungia, found fixed to an anthocormus, has a diameter of 5 cM.

Besides the budding at the anthocormus, also lateral budding occurs in *Fungia actiniformis*. The method of lateral budding, occurring so frequently with *F. fungites* when the tissues of this species are partially destroyed by algae, seems to be very rare in *F. actiniformis*. I found only one specimen, exhibiting this mode of budding. Three fourths of this Fungia was defunct. Only the remaining fourth was covered with living tissue and bore tentacles. There is a bud about midway between the border and the centre at the underside on the boundary between the defunct and the living part, still in the latter. The septa of the bud, arranged radially, are modified spines, but much larger than those of the environment and distinctly flattened. The septa are over their full length attached to the underside of the parent-coral, a stem has not yet been formed.

Another very peculiar mode of budding seems to occur rather frequently in *F. actiniformis*; I found near Edam 10 specimens which exhibited it.

These buds occur at the underside, attached to the scar by which the coral had been fixed in its young state to the stem of the anthocormus (fig. 6). This scar is covered with living tissue, which proceeds into the tissue of the bud. Tentacles are distinctly noticeable

[1]) TH. STUDER, Uebersicht der Steinkorallen aus der Familie der *Madreporaria aporosa, Eupsammina* und *Turbinaria*, welche auf der Reise S. M. S. Gazelle um die Erde gesammelt wurden. Monatsber. K. Preuss. Ak. der Wiss. Berlin 1877.

in the buds, which for the rest produce a normal impression, only the soft parts are of a lighter colour than those of the upper side

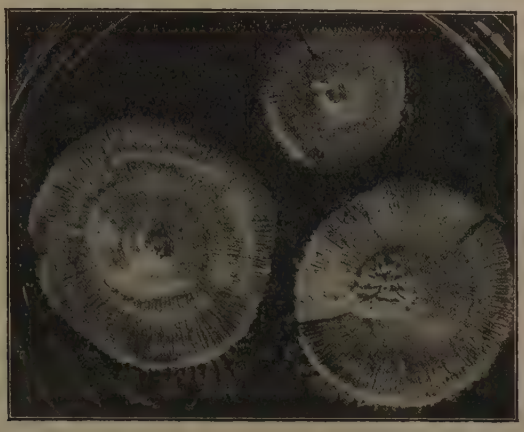

Fig. 6. *Fungia actiniformis.* Underside of three living specimens with budding at the scar.

of the parent. Since the buds occur at the underside, they are shut off from the light, to which the lighter colour of their tissue is perhaps to be ascribed. This budding generally presents one bud at the scar, sometimes two. With the older buds of this kind the upperside is already distinctly broadened into a disc, so that they have short stems. The structure of the skeleton is regular like that of the young buds of anthocormus, but it is very thin and fragile. It is difficult to account for the origin of these buds; the specimens in which they occur are already mature, with a transverse diameter of more than 5 cm. and for the rest look quite normal. Neither have they suffered from algae-parasitism, which consequently cannot have given rise to this budding. Maybe these buds are loosened later on, and are located under the disc of the parent-coral, which brings about a very unfavourable condition.

Excepting the formation of buds at the anthocormus budding in *Fungia fungites* and *F. actiniformis* is ever an abnormal phenomenon. In nearly every case in which buds could be observed, they could be shown to originate from an increased growth of the tissue owing to seaweeds or other organisms which established themselves here. Only one category of buds forms an exception viz. the buds on the scar of *F. actiniformis*.

This scar, in fact, is the place of an old wound, but even very young Fungiae, recently dropped from the stem, have covered this cicatrice again with living tissue. It is, therefore, difficult to account

for the origin of this renewed activity of growth at the scar, which induces the formation of these buds. Though their aspect is normal and regular, they are in unfavourable conditions for further development.

The most successful mode of budding in adult Fungiae is no doubt that from the remains of the living tissue of a disc of *F. fungites* that is almost entirely overgrown with seaweeds or other organisms.

Here the buds have been formed as normally as those of an anthocormus. On this account many researchers consider these buds to have directly arisen from larvae.

At an anthocormus a large number of buds are massed together within a short time. Most of them form new buds laterally to their stem. Now when the anthocyathus of the buds gradually enlarges, this broadened extremity often leans against the disc of a neighbouring young Fungia, which inhibits further broadening in those places. In this way originate anomalous young Fungiae, as may be seen from many colonies. Hereby the anthocyathus is elongated in many cases in one direction or is angular with many flattened sides.

This close contiguity may also cause the undersides of two young Fungiae to coalesce, the buds then drop simultaneously and remain twinned. At an anthocormus of *Fungia actiniformis* I found two of these young buds, the underside of one of which was at one place grown together with the other. The septa of the one anthocyatus are still separated from those of the other. During the transport these buds got loose from their stems but they were not severed from each other.

Not unfrequently do we find old Fungiae, which clearly show their origin through coalescence of two buds as is evident from two scars at the underside of such twin-specimens. When these twins have arisen from the intergrowth of two Fungiae of about the same age two mouths with the surrounding septa are to be observed at the upperside, the septa being grown together anomalously at the plane coalescence between the two individuals.

Now the occurrence of two or more mouths at the upperside of a Fungia would not warrant the conclusion that such a coral has arisen from several individuals, for when, for some reason or other, a stronger growth appears in one part of the border than in the other part, folds will make their appearance which may extend upwards over a pretty long distance, as a doubled up border. If this folding process continues up to the mouth, it often results in a

splitting of the primary mouth into smaller ones, while the septa formed afterwards then arrange themselves radially round these new mouths. Near Edam I found similar specimens of *F. fungites*

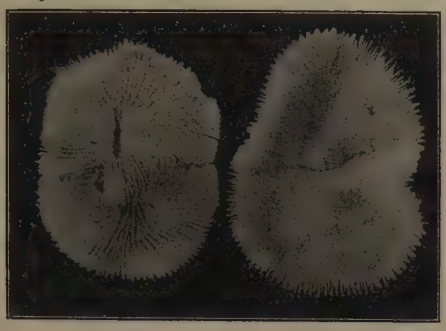

Fig. 7. *Fungia fungites.* Twins, the result of coalescence of two buds of an anthocormus. To the left a specimen seen from above; to the right a specimen seen from below. 2/5 natural size.

as well as of *F. actiniformis*, but when examining the underside we clearly see that only one individual is concerned here, as only one scar is observable. [1])

Rather considerable divergencies in the size of the intergrowing buds may produce formations which remind us of budding at the underside of an adult individual, as is very beautifully typified in a specimen of *F. actiniformis* that I found near Edam. At the underside of this Fungia of 10 cm. diameter a young coral of the same species of 4 cm. diameter is partially grown together with it.[2])

The septa of the smaller Fungia, facing the centre of the larger one, are ill-developed as they touched the ground. The other side possesses well-developed septa and in a living state, bore long tentacles, so that the mouth is rather remote from the centre. The larger Fungia has developed into a normal individual, the smaller one was covered by it entirely and was moreover partly overgrown with sea weed, which also blunted the sharp edges of the scar.

In *Fungia actiniformis* there is generally at the underside in the centre a truncated conical platform, of which the flattened surface constitutes the most often sharply outlined scar of attachment to

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[1]) QUELCH (l. c. page 131) also records the occurrence of similar abnormal individuals.

[2]) The ribs of the smaller Fungia are grown together at the indicated place with those of the larger one: if this were a case of budding, these ribs would grow by themselves.

the anthocaulus. Now we sometimes observe a specimen with stemmed buds by the side of this coniform part. I found one of 43 mm. in diameter; which bore laterally to the platform, at the underside, two buds respectively 3,5 and 3 mm. in diameter; the peripheral portion of the stem had not yet broadened into a disc. From the basis to the septa these buds measured respectivily 6 and 5 mm. These buds completely resemble young Fungiae of an anthocormus; they usually occur at the stem of an older bud as lateral branches. In normal cases they are seen below the spot where afterwards the young Fungia will drop from the stem, so that they can develop further, when this takes place. The instance described goes to show that sometimes the tissue above the preformed cicatrix also engenders buds, which however, stick to the underside of the young Fungia, when the final bud drops from the anthocaulus and which are hereby impeded in their further development. The above interpretation seems to me more plausible than the hypothesis which represents the problem as a lateral budding, arisen after the young Fungia has detached itself from the anthocormis. The size of this coral points to its having only just dropped from the stem.

In *F. actiniformis* the scar has a sharp edge, the boundary between the scar and rest of the underside remains sharp also with older specimens of this species. In *F. acitiniformis* this facilitates the decision whether an apparently coalesced specimen has originated from two buds or through abnormal growth of one specimen. The scar of *F. fungites* becomes obscured in older specimens, in little ones it is mostly easy to distinguish. In the specimens of Fig. 7 the scars could easily be noted, so that this is an indubitable case of coalescence on the anthocormus. Fixed, stemmed coalescent anthocyathi of *F. fungites* I have not been able to discover.

From the Treub Laboratory Buitenzorg, Aug. 1921.

Physiology. — "*On the Movement of Pepsin in a protein-containing or protein-free Gel of Agar-agar*". By Prof. C. A. PEKELHARING.

(Communicated at the meeting of November 26, 1921).

In an earlier paper [1]) presented to this Academy I dwelt on a peculiar protein obtainable from the gastric mucous membrane, which could be procured in a purer condition from a dog's gastric juice that is neither contaminated with swallowed matter nor with the constituents of the intestinal contents, so that the elementary analysis did not bring forth greater differences than are generally found with purified proteins. This peculiar protein evinced the properties of pepsin in such a marked degree and the digesting power was in different preparations so constant, that I felt justified in supposing that this protein could be the enzyme itself. Subsequent investigations have repeatedly confirmed this view.

However, in discussing the nature of enzymes with our fellow member BEIJERINCK he raised an objection against this conception. According to his experience pepsin, or chymosin (which enzymes I hold to be identical) diffuses in agar-agar about as quickly as albumoses.

This was, indeed, a serious objection. The pepsin, as I prepare it, is split while being rapidly heated in an acid solution to the boiling point, so that albumoses which remain in solution, are liberated, while a considerable precipitate is being formed, from which, on heating with potassium hydrate, part of the sulphur is freed together with substances yielding a biuret-reaction. With acid a precipitate of a new protein can now be obtained from the alkaline fluid, which protein possesses comparatively energetic acid properties and is soluble in alcohol. This pepsin, then, is of a much higher composition than the simple proteins grouped under the name of albumoses. If my conception were correct, pepsin would surely not diffuse so easily as albumose in a gel of agar-agar.

However, I put myself the question whether the movement of the enzyme is indeed to be ascribed entirely to diffusion. Might

1) Proceedings of the Meeting of 25 Jan. 1902, p. 450.

there not be another cause, if the pepsin were to find in the gel protein which it could attack?

It has long been known that enzymes are capable of binding other substances, not only substances which can be decomposed under the influence of the enzyme, but also substances of quite a different nature, on which the enzyme does not exert any influence at all. Pepsin e.g. combines not only with proteins but also with carbon.

In a concise review on the nature and the action of enzymes[1]) I have endeavoured to show that this combination is effected in various ways. First of all there is adsorption. If, as in the case of pepsin, the enzyme and the substance bound by it are both colloidal substances and consequently a difference in surface-tension is of little importance for the adsorption, it is especially the difference in the electric charge of the molecules that comes into play. Owing to this the particles of one substance aggregate as closely as possible on the periphery of those of the other substance. In an acid solution pepsin is charged negatively, protein positively.

The compound thus formed is to a large extent independent of the nature of the two substances. Just as finely divided carbon can bind pepsin as well as all sorts of other enzymes, trypsin also combines not only with protein but also with starch and compounds have been obtained of amylase not only with starch, but also with casein. Adsorption promotes the action of the enzymes by increasing the concentration of the substrate in the immediate neighbourhood of the enzyme or the concentration of the enzyme in the immediate environment of the particles of the substrate. This action is, however, only of a promotive character. For a chemical change the enzyme must combine with it in a manner that depends on the molecular constitution of the substrate as well as of the enzyme. As E. Fischer has put it: the enzyme must fit to the substrate, or what Beyerinck terms the "zymotele", like a key to a lock. Only when this kind of combination is effected, can the decomposition of the substrate, generally with addition of water, take place. In this process the enzyme is detached from the substrate in order to combine again with other still intact particles of it. Consequently a small amount of the enzyme can continually decompose new particles of the substrate, unless the enzyme itself is destroyed by noxious influences, as e.g. is the case with trypsin by alkaline reaction of the solution, which however aids the action of the enzyme.

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[1]) Some Remarks on Enzymes. Recueil des Trav. Bot. néerl. XVI, 207.

In this connection we are induced to suppose that the particles of the enzyme are continually moving, while incessantly particles of the substrate are being decomposed. So, when a certain amount of pepsin is going to spread into the protein-containing gel through diffusion, the particles of the pepsin will first be bound to the protein-molecules by adsorption, through the difference in the charge. If only adsorption should come into play, a large number of the pepsin-molecules would be detained, without undergoing or causing any change, while the subsequent diffusion would be inhibited rather than accelerated. It makes a great difference, however, if in virtue of their constitution the molecules of the enzyme also grasp the protein-molecules and they attach themselves to new intact protein-molecules with which they come into contact, after the splitting of the protein and, consequently, because free protein-particles are lying on the periphery, they move towards the periphery, and — seemingly — quicken the diffusion.

Now in the experiments which gave rise to BEIJERINCK's objection the agar-gel contained protein. The question, therefore, was, whether in such a gel, ceteris paribus, the spreading of the pepsin would be quicker in the presence of protein than in a gel without protein, in which only true diffusion would take place.

— To my friend and successor Prof. W. E. RINGER I feel greatly indebted for his highly appreciated help in my endeavours to find an answer to this question in his laboratory. The inquiry was conducted as follows:

25 mgrs of purified pepsin from the pig's gastric mucous membrane was put in a test-tube, dissolved at body-temperature in 2,5 cc. 0,2 % HCl, and subsequently 2,5 cc. 3% agar-agar in water was added. By rapid shaking the pepsin was evenly mixed up with the heated agar-agar and immediately after cooled down in melting ice. The small clots of coagulated agar sticking to the wall of the tube consequent on the shaking, were whisked cautiously away and, in order to destroy all the pepsin that might be left behind in the tube above the coagulated column, the tube was filled with 1 % NaHO, then emptied after some moments and washed out a couple of times with water and afterwards with 0,1 % HCl. After this 10 cc. of a mixture of 5 cc. agar-agar 3% and 5 cc. 0,2 % of HCl to which protein was added or was not, was put into the tube. Then the tube was cooled down again in ice. In each experiment four tubes were filled in this way, two with and two without protein. They were then closed with a cork stopper, placed vertically in an incubator that was kept at 27° C.

The amount of pepsin in the lower part of the tube was very considerable (25 mgr.), while 0,1 mgr. of this enzyme in 10 cc. 0,2% HCl dissolves in METT's tubes from 5 to 6 mm. of coagulated white of a hen's egg in 24 hours at 37° C. What was lost of it in mixing the pepsin with the heated agar (which was directly after cooled down in ice) and what was lost in the washing of the tubes with sodium-hydrate, through which of course also a little of the pepsin at the surface of the pepsin-agar column was attacked, could only be very insignificant in relation to that considerable amount of pepsin. It was assumable, therefore, that the concentration of the enzyme in the reservoir sufficed to prevent in the several tubes considerable differences in the degree of the rise of the enzyme in the agar-column above the pepsin-agar.

After a few days every time two tubes were opened, one with and one without protein. To this end a circular incision was made into the glass just on a level with the boundary between the pepsin-agar and the column above it and the glass was broken by touching it with a heated rod. The lower part of the tube could then readily be removed and the whole content be slid out and put on filter-paper.

It might be that the fluid in the capillary spaces between the agar and the glass should have taken up more or less pepsin from the pepsin-agar: a possibility which deserves the more consideration as occasionally it could be observed at the free surface of the column that some fluid had been pressed out, which could dissolve fibrin, though it be in a very small degree. That is why after the reservoir of pepsin had been cut off from the agar-column, this column was immersed for some moments in 1% Na_2CO_3, then washed immediately in 0,1 % HCl and dried by cautiously rolling it along filterpaper.

We now had to determine the level to which the pepsin had penetrated into the agar-column. With a view to this we proceeded as follows: after cutting off a layer of 2 mm. thickness, there where the column had been in direct contact with the pepsin-agar, the column was divided into three cylinders of equal length, mostly 13 mm. in length, sometimes 15, if the diameter of the tube had been somewhat smaller, and if the whole column had consequently been somewhat longer. In this division we started from the bottom, so that the layer nearest to surface could be rejected. The cylinders were weighed, rubbed down in a mortar with 5 cc. 0,1% HCl. For every one of these fluids we now determined the time in which 1 cc. coagulated 5 cc. of milk at 27° C. We ascertained the com-

parative proteolytic power, after the method of GRÜTZNER [1]) by mixing 1 cc. of the fluid with 9 cc. 0,1% HCl to which, at least 10 minutes before, 50 mgrs of finely divided carmin fibrin had been added, then turning the tube once every minute, filtering the contents after a certain time through glasswool and establishing the intensity of its colour with the aid of GRÜTZNER's colorimeter against a solution of carmin fibrin in pepsin hydrochloric acid.

First of all the proceeding of the enzyme was compared in agar-agar with and without fibrin. With a view to this in each of two tubes, containing agar-agar, 10 CC. was added of a mixture of equal portions of 3% agar and 0,2% HCl, and in two others 10 CC. of a mixture of 3% agar and 0,2% HCl with carmin-fibrin that had been rubbed down very finely and had swollen in this acid.

After three days one of each couple of tubes was opened and examined in the manner described. Just as in all the following experiments **I** designates the lowermost cylinder, the one nearest to the pepsin-agar; **II** the one next to it and **III** the topmost cylinder.

The result was to this effect:

	Weight (grms)	Milk clots in	Division mark Colorimeter	Weight (grms)	Milk clots in	Division mark Colorimeter	Weight (grms)	Milk clots in	Division mark Colorimeter
with fibrin	I 2.04	2 min.	1.2	II 2.14	15 min.	0.5	III 2.0	30 min.	not measurable
without »	I 1.8	4 »	1.2	II 1.5	no clotting	0	1.5	no clotting	0
After 6 days the other two tubes were opened. Now we found:									
with fibrin	I 2.48	3½ min.	1.7	II 2.54	10 min.	0.8	III 8.24	90 min.	not measurable
without »	I 2.04	4 »	1.0	II 2.04	no clotting	0	III 2.0	no clotting	0
The 2nd experiment was conducted in the same way. Result after 4 days:									
with fibrin	I 1.80	2 min.	4.1	II 1.95	17 min.	1.1	III 2.00	40 min.	0.5
without »	I 2.10	2½ »	3.1	II 2.00	18 »	0.5	III 2.00	no clotting	not measurable
After 13 days:									
with fibrin	I 1.20	1¾ min.	2.8	II 1.54	4 min.	2.4	III 1.60	17 min.	1.2
without »	I 1.66	2¼ »	2.6	II 1.94	14 »	1.2	III 1.90	60 »	0.1

[1]) Vide GESELSCHAP, Zeitschr. f. Physiol. Chem. XCIV, 205 and Onderz. Physiol. Laborat. Utrecht, 5e R. XVI, 198.

I have desisted from a determination of the absolute pepsin amount of the several columns, though this could be done by comparison with a solution of pepsin of known strength, since the pepsin in each column was not divided evenly, but lessened considerably from the bottom upwards. The values given show distinctly enough, that the enzyme rose in the agar without protein as well as in the agar with protein, but in the latter more considerably. Only after 13 days could it be demonstrated that the enzyme had reached the upper column in the protein-free gel.

As regards the absolute value of the figures it will not do to compare the results obtained on various days, because the milk used was different every time and for comparison in the colorimeter every time another solution of carmin-fibrin was taken.

Similar results were achieved with clotted white of a hen's egg:

White of a hen's egg, diluted with 10 times its volume of water, was beaten up and coagulated by boiling under addition of acetic acid to a very weak acid reaction. The flaky precipitate was filtered off and washed with water. Part of this was put in 0,2 % HCl and evenly distributed in the fluid by rapid shaking. In each of two tubes with pepsin-agar was added 5 CC. of this protein-containing acid, mixed with 5 CC. 3 % agar in two other tubes 5 CC. 3 % agar with 5 CC. 0,2 % HCl.

Two tubes examined after 3 days:

	Weight	Clotting	Colori-meter	Weight	Clotting	Colori-meter	Weight	Clotting	Colori-meter
with protein	I 2.64	1¾ min.	2.2	II 2.56	22 min.	0.4	III 2.35	40 min.	very light red
without »	I 2.70	2 »	2.0	II 2.56	130 „	not measurable	III 2.40	none	0

The second set of two tubes got lost.

In every tube so much hydrochloric acid had been put, that the content of the gel was 0,1 % over the whole tube. Here, however, we had to consider that in the tubes containing the protein, the acid was partly bound, so that the concentration of the H-ions in the agar-protein gel was undoubtedly lower than in the agar gel without protein. The observed differences could, however, hardly be attributed to it. If the movement of the enzyme depended exclusively upon diffusion, it might presumably be promoted by an acid reaction, considering that, during the sojourn of the tubes in an environment of 27° C., the acid attacks and softens the agar. In every experiment therefore, the protein-containing agar was more solid than the protein-

free agar, nay in some experiments the protein-free agar had become so soft that it could not be divided into three small cylinders so that comparison with the protein-containing agar was impossible.

In accordance with this we also found, that the enzyme proceeds in agar with casein more rapidly in acid-, than in neutral reaction.

A 3% neutral solution was made of pure casein prepared after HAMMARSTEN by addition of NaHO. A part of this was diluted with an equal volume of water, another part with an equal volume of 0.4% HCl. The precipitate arising primarily on the addition of hydrochloric acid was dissolved again in the excess of acid.

Of this neutral solution 5 CC was put in two tubes filled with pepsin-agar, and was mixed with 5 CC 3% agar. In two other tubes 5 CC of the acid solution mixed with 5 CC 3% agar.

After 3 days:

	Weight	Clotting	Colorim.	Weight	Clotting	Colorim.	Weight	Clotting	Colorim.
acid	I 2.27	3 min	4.5	II 2.30	none	0.6	III 2.24	none	not measurable
neutral	I 2.25	2 „	5.6	II 2.30	none	0.3	III 2.30	none	0

After 4 days:

acid	I 2.32	1¼ min.	4.4	II 2.30	10 min.	1.6	III 2.33	13 min.	1.3
neutral	I 2.30	1½ „	4.2	II 2.33	none	0.3	III 2.30	none	0

In both sets of tubes, then, there was a balance in favour of the acid solution. After 4 days pepsin could even distinctly be observed in the top cylinder of the acid gel.

Also with a neutral reaction the movement of the enzyme through the gel was aided by the presence of protein, as appeared from an experiment with milk.

Of 4 tubes containing pepsin-agar two were supplied with 3 CC milk mixed with 7 CC 3% agar; the other two with 3 CC 1% NaCl mixed with 3 drops of 1% $CaCl_2$ and 7 CC 3% agar.

We observed that also globulin from blood-serum and from edestin had a favourable action on the movement of pepsin through the agar-gel.

Finally I report some more experiments which I carried out to verify the supposition from which I started, viz. that the advance of the pepsin in the protein-containing gel is promoted, because besides

After 3 days:

	Weight	Clotting	Colorim.	Weight	Clotting	Colorim.	Weight	Clotting	Colorim.
with milk	I 2.24	2 min.	3.0	II 2.24	4 min.	2.1	III 2.20	9 min.	0.5
without „	I 2.20	4 »	2.1	II 2.40	2¾ hour	not measurable	III 2.30	none	0

After 4 days:

with milk	I 2.25	2 min.	1.7	II 2.36	3 min.	1.5	III 2.40	20 min.	0.8
without „	I 2 50	2½ »	1.6	II 2.50	1 hour	0.4	III 2.25	none	not measurable

through adsorption it combines with the protein still in another manner in consequence of the chemical structure of the molecules. When this compound breaks down, in which process the action of the enzyme manifests itself, the liberated enzyme is supposed to attach itself to other still intact protein molecules which are lying on the periphery and to advance in this way in the direction of the diffusion-current. If this supposition is correct, the movement of the pepsin must also be promoted by albumoses, which it is still able to attack; not, however by animo-acid freed from the protein which pepsin cannot attack and which, in contradistinction to albumoses, it cannot grasp[1]) in an electric field.

To this end we mixed, in the manner described above, first, primary and secundary albumoses, prepared by digestion of fibrin with gastric juice, and then a mixture of pure amino-acids approximately in the relation in which they are contained in fibrin, in a solution of 0.2 % HCl, with the same volume of 3 % agar-agar.

The primary albumoses contained a considerable amount of heteroalbumose, the secondary ones were freed as much as possible from primary ones by repeated half-saturation with ammonium-sulfate and by filtration.

Primary albumoses. Two tubes each with 100 mgrs. of albumose, two without albumose, prepared as usual.

It appears then that, while the primary albumoses largely promote the movement of the enzyme, the action of the secondary ones, which are much less attacked by pepsin, though it cannot be entirely denied, is much less significant.

Nothing, however, could be detected of an action of the amino-acids, as is shown by the following experiment:

[1]) Vide Ringer, Zeitschr. f. Physiol. Chem. XCV, 195 etc. Onderz. Physiol. Laborat. Utrecht. 5de R. XVI, 252.

After 3 days:

	Weight	Clotting	Colorim.	Weight	Clotting	Colorim.	Weight	Clotting	Colorim.
with album.	I 2.35	1 min.	3.3	II 2.40	7 min.	2.1	III 2.40	22 min.	1.0
without »	I 2.20	1¼ »	3.2	II 2.20	35 »	1.0	III 2.15	none	0

After 4 days:

with album.	I 2.05	50 sec.	3.5	II 1.95	2 min.	3.2	III 1.98	4 min.	2.5
without »	I 2.04	1 min.	3.1	II 2.00	6 »	1.2	III 2.08	none	0

Deutero-albumoses, 100 mgrs.

After 2 days:

with album.	I 2.40	1¼ min.	5.5	II 2.44	15 min.	2.5	III 2.44	1 hour	1.0
without »	I 2.40	1½ »	4.5	II 2.34	1 hour	1.0	III 2.30	2 hrs	0.6

After 3 days:

with album.	I 2.24	1 min.	3.0	II 2.10	2½ hour	0.5	III 2.14	none	0
without »	I 2.20	1¼ »	3.0	II 2.34	none	0.2	III 2.20	none	0

The solution contained in 11 CC 0.2 % HCl, 75 mgrs. tryptophan, 7.5 mgrs. of cystin, 40 mgrs. of histidin, 70 mgrs. of tyrosin and 30 mgrs. of alanin. On heating to 40° C. the solution was almost clear. Of this solution 5 cc. and 5 cc. of 3 % agar was put in each of 2 tubes. In the other 2 tubes 5 cc. of 0.2 % HCl was put, together with 5 cc. of agar.

In the tube heated for 4 days at 27° C. the gel without amino-acids, which, therefore, had been more exposed to the action of the acid, was very soft. Perhaps it is owing to this that the pepsin has penetrated farther than is generally the case in the agar without protein.

It might be surmised, that from the experiments described it does not even follow that pepsin is indeed competent to diffuse in pure agar, seeing that a gel of this agar prepared in the usual way, will always contain nitrogenous substances, which may belong to the group of proteins. I believe this is an unjustifiable assumption. It is difficult to ascertain whether the agar-gel or sol contains protein because sensitive reactions on protein cannot be successfully applied.

After 3 days:

	Weight	Clotting	Colorim.	Weight	Clotting	Colorim.	Weight	Clotting	Colorim.
with am.-acids	I 2.27	1 min.	5.5	II 2.28	22 min.	0.5	III 2.80	none	0
without »	I 2.10	2½ »	3.1	II 2.20	none	0	III 2.16	none	0

After 4 days:

with am.-acids	I 2.35	1½ »	3.5	II 2.30	2 hour	0.3	III 2.30	none	0
without »	I 2.16	1 »	4.5	II 1.85	8 min.	1.7	III 1.70	25 min.	0.8

here, owing to the dark colour caused by the action of strong mineral acids on the carbonhydrate. It is possible, however, to remove the greater part of the nitrogenous substances by warming the agarsol during 24 hours at about 50° C. The nitrogenous substances will then separate in flakes, so that they can be filtered off. In this way I obtained from an agarsol a sol which was scarcely opalescent and remained almost clear also after the clotting. The agarsol had been prepared in the usual way by warming it just sufficiently and then filtering it through cottonwool. It contained 1.6 % N of the solid substance. In the sol there was only 0.39 % N after heating during 24 hours and filtering through compressed paper pulp at about 50° C. This gel was now compared in the usual way with the one that had been filtered only once, to the effect that there was no difference to be observed in the advance of the pepsin. In the gel containing only very little N we could make out in the lowermost cylinder, which had been situated at a few millimeters distance from the pepsin-agar, as much enzyme as in the gel which contained four times that quantity of nitrogen.

That pepsin had no doubt advanced through diffusion. But this movement is very slow.

Whereas after a few days a rather considerable amount of pepsin has penetraded from the reservoir at the lower portion of the tube into the adjoining agar, there is none or hardly any to be made out at a few centimeters distance, anyhow, if the gel has preserved its compactness. If, however, the gel contains protein, the enzyme has proceeded much further in the same space of time.

In my opinion the foregoing warrants the conclusion that the movement of pepsin through a gel which contains protein it is able

to attack, does not entitle us to doubt that the size of the pepsin-molecule is as great as previous observations have assigned to it.

On the other hand it seems to me, that the bearing of protein on the movement of pepsin through a gel, favours the hypothesis that the combination of an enzyme with the "zymotele" is not to be ascribed only to adsorption, but also to a totally different action depending on the structure of the molecules.

On starting this inquiry I purposed to extend it in various directions and over more enzymes, notably invertin and emulsin, which can attack various carbonhydrates of known structure. But I understand that my time for Laboratory work is passed. I must now leave this to younger workers who consider this subject interesting enough to investigate it further in the indicated direction or in their own way.

Physiology. — *"The Myoclonic Reflexes"*. By L. J. J. MUSKENS. (Communicated by Prof. H. ZWAARDEMAKER).

(Communicated at the meeting of December 23, 1921).

Under the influence of intoxication (camphorum monobromatum, essence of absinth) the myoclonic reflexes may be seen to shorten their normal long refractory stage to a small interval. For the rest the tracing presents the type of the patellar reflex, a rather steep beginning with a protracted termination.

The myoclonic reflexes vary in some respect with the stimulus that elicits them.

Those provoked by tactile- and acoustic-stimuli have a latent period, which is comparatively short; my measurements of this non-reduced reflex-time were 20—60 δ. It appeared that for the tactile reflexes no important changes in the latent period were brought about neither by camphorum monobromatum intoxication (with extension of the reflex-movement over the whole animal), nor by ether-narcosis, nor by removal of a hemisphere.

While with most reflexes a slight decrease of the reflex-time is noticeable with *stronger* stimuli, I found in three experiments with cats, specially carried out for this purpose, (in a definite latitude of intensity), a longer latency with a strong acoustic stimulus than with a weak one.

Latency after acoustic stimulus in $^1/_{50}$ sec.

Cat. N°.	Light blow	Moderate blow	Heavy blow
205	1.6.	2.5.	2.1
178	1.8. en 1.9.	2.6. en 2.6.	2.1

The acoustic reflexes differ from the tactile reflexes also in other respects. In ether-narcosis a lengthening of the latency of the acoustic reflexes is observed; while the acoustic reflex disappears sooner in the narcosis than the tactile reflexes and also returns later. The latency of the myoclonic reflexes in other animals presents only few divergencies from the values found in cats and monkeys. Tactile stimuli administered to pigeons at the wing gave 2.5 and 3.5 fiftieths of seconds. For ink-fishes (Octopus) I found, after adding some camphorum monobromatum to the seawater, similar latencies; in reptiles in a normal condition four fiftieths (Trepidonotus natrix). Just as everywhere in the study of reflexes we find in the myoclonic

reflexes oscillations in latency and in magnitude of the reflex, which, however, are not more considerable than elsewhere.

The reflex-convulsion is in normal conditions restricted not unfrequently to the affected part of the body; if the latter is a limb, the adductors are the first to react. In case of a greater reflectory excitability the reflex involves the whole voluntary muscular system, trunk, and extremities in the process. The propagation of the reflectory effect on the other parts of the body, either through modification of the stimulus, or through increased reflectory excitability, is not gradual but abrupt, in so far as with acoustic stimulation (e. g. clap of the hands) the myoclonic reflex movement is first restricted to the head, then on a slight intensification of the stimulus is transmitted rather abruptly to the fore-limbs, and on a second slight intensification comprises the whole trunk and the extremities. It seems, then, that the centra, which govern the co-operation of the several parts of the body, are not involved in the reflex the one after the other but all together. The rule "all or nothing" seems to hold also here within certain limits. The reflex-convulsion which involves head, legs and trunk, produces an impression as if all the parts of the body are contracted at the same moment. With the aid of separate tambours on the head and the trunk distinct differences are observable, which are evidently connected with the path along which the stimulus proceeds. The subjoined registration of the averages, determined in experimenting with cat 201 after a slight dose of camphorum monobromatum, may serve as an example:

Reflex-time in $^1/_{50}$ sec.

Stimulus	Reflex-movement of the head	Movement of the back
Gong.	1.7.	2.2.
Tap on the back.	2.5.	2.6.
Tap on the tail.	3.1.	3.3.

When in such a series of experiments the stimulus is gradually intensified, the reflectory effect will be seen to increase irregularly, until at a given moment a very marked reinforcement of the effect is brought about in the form of an "after-discharge", invariably in the form of a series of convulsions. The extension of the reacting region does not at all take place according to PFLÜGER's law. In applying the stimulus to the trunk, it is the head that convulses first. Under all circumstances the effect is much stronger when the stimulus is given unexpectedly and without being seen.

Geology. — *"On the hot "Lahar" (mud flow) of the Valley of Ten Thousand Smokes. (Alaska)"*. By B. G. ESCHER. (Communicated by Prof. G. A. F. MOLENGRAAFF).

(Communicated at the meeting of November 26, 1921).

After numerous expeditions to the *Mt. Katmai* volcano and the "Valley of ten thousand smokes" in Alaska, ROBERT F. GRIGGS has amply communicated his observations (lit. 1—6).

Particularly he has viewed the great hot mud flow which occupies an area of 137 sq. Km. in the valley of ten thousand smokes.

As he points out (lit. 5, p. 142) [1]) he specially treated this phenomenon so extensively because he felt obliged to give an explanation which would be a novelty in volcanology.

We should be very grateful for his frankness and for the opportunity he has afforded his colleagues to venture on an other explanation of the great hot mud flow.

It is evident that GRIGGS is unaware of the nature of the volcanic explosions of the *Klut* volcano in Java and it seems to me that this volcano gives us the key for the disentanglement of the enigma which was posed before GRIGGS.

We may summarize the observations and explanations of GRIGGS as follows [2]).

A couple of valleys with an aggregate length of 32 Km. are covered by a hot mud flow, which left practically, everywhere a "high water-(mud-)mark" on the surrounding slopes of the mountains. Stratified ash from the explosion of Mt. Katmai in June 1912

[1]) In the discussion of this remarkable terrane we have set down numerous considerations which would be quite superfluous if it were located in a district more accessible to geologists, so absolutely clear are its major relations. But, recognizing that under present circumstances it would not be practicable for all geologists who might be skeptical to go and see it for themselves, we have tried to supply the answers to all the questions likely to arise in the minds of such skeptics" (lit. 5 p. 142).

and:

„I would further add that I am not committed to any theory of the origin of this curious terrane, but will be glad to accept any other interpretation that can be suggested, provided only that it is consistent with the facts as found in the field. Certainly any suggestion that would relieve us of the necessity of postulating an entirely new type of volcanic action will be most welcome" (lit. 5, p. 119).

[2]) For the present I do not take into consideration the most recent version of GRIGGS (lit. 6).

rests directly on the tuff of the mud flow. Moreover this stratified ash covers a more extended area and on the South slope of Mt. Katmai there lies upon this ash a cold mud flow. This cold mud flow is explained by GRIGGS as the result of heavy rains, with which explanation I fully agree. The cold "lahars' from the *Smeru* volcano (Java) which brought about the notorious disaster of *Lumadjan*, are one of the many parallels to this cold mud flow (lit. 11).

GRIGGS seems to be unaware of other mud flows from volcanic material than those caused directly by rainwater or by the thaw water from glaciers [1]), but there are sufficient indications in the Katmai region that the glaciers did not melt to any great extent in connection with the explosion of Mt. Katmai. GRIGGS has estimated the cubical capacity of the tuff of the great hot mud flow as one cubic mile (lit. 5 p. 137), or, in other words, as 4096 million cubic metres. To hold this quantity of solid material in suspension he esteems necessary at least as much water, i.e. at least 4096 million cubic metres, and he has not succeeded in finding a source for this enormous quantity of water.

Moreover he has been unable to account for the fact that the great mud flow must be older than the ashfall, although according to his views the ash deposited as subaërial sediment was caused by the explosion of Katmai.

If the strata of ash lay below and the mud flow rested upon it, it is assumed by GRIGGS that the matter is capable for simpler explanation, but this is by no means the case, for the reason that the great mud flow was hot. Since he could not find water coming from above GRIGGS has concluded that the mud which formed the great mud flow welled up from within the valley itself. His view is that the mud welled quietly up without any explosive action through several fissures in the floor of the valley.

GRIGGS is well aware that he has imagined a volcanologic phenomenon wholly unknown to science and declares that he is unable to suggest any hypothesis to account for the mecanism which brought about the great mud flow.

His conclusion he has talked over with some students of volcanism in America, who after the most violent opposition have accepted, as he says, his interpretation as the only one in harmony with the facts.

From this it would appear that the numerous publications on the *Klut* volcano (lit. 7—15) are not well known in America.

The nature of the outbreaks of the *Klut* volcano, which is known

[1]) These mud flows are known in Iceland by the name „jökulhlaup" (lit. 18 p. 171).

from numerous explosive eruptions may be summarized as follows.

After each eruption during an average period of rest of 18 years [1]) the crater is after about six years filled up with rainwater. Hence the new explosive eruption must take place through the water of the crater lake, so that this is blown out of the crater mixed with hot ash, pumice and volcanic bombs and lapilli all of which flow down the slopes of the volcanic cone as a hot mud flow, which is known as a "*lahar*" (hot lahar) by the Javanese people.

Eruptions of the Klut-volcano are known to have taken place in 1586, 1752, 1771, 1811, October 11—14th 1826, 1835, May 16th 1848, January 4th 1864, May 22th—23th 1901 and May 19th—20th 1919.

In 1875 without any volcanic action the west side of the rim crumbled away, by which the crater lake was partially emptied and cold lahars were formed.

When the eruption lasts longer than the time necessary to blow out all the water from the crater lake, there follows upon the hot lahars an ordinary ash-rain. Still later cold lahars follow in consequence of the abundant tropical rainfall.

The section of deposits which results from the above sequence would comprise in order from above downward:

III. Tuff from cold lahars in consequence of heavy rainfall. (Transposed material from I and II).

II. Ash, deposited as subaërial sediment. (Second eruption-sediment).

I. Tuff from hot lahars. (First eruption-sediment).

It is remarkable how much heat remains in such a lahar after the water has partly drained away and in part evaporated. After the eruption of 1919 KEMMERLING (lit. 14) observed in numerous places pseudo-volcanic phenomena, caused by the water evaporating in the lahar [2]). VISSERING (lit. 11 p. 73) mentions that a walking-stick which four days after the eruption of 1901 was poked into the lahar, was drawn out in flames. KEMMERLING mentions (lit. 14 p. 811) that some days after the eruption he measured a temperature of 360° C. in a gas-emanation on the lahar. March 25th 1921 I visited the Klut and Mr. G. K. R. HÖJGAARD, topographer to the Netherland East Indian Mining Service (Mijnwezen) told me that the temperature of the lahar of May 1919 still amounted to 178° C. at a depth of 50 cM.

The final account of the Klut eruption of 1919 by the volcano-

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[1]) This average is computed from the eruption dates since 1811.

[2]) A splendid photograph of the lahar of 1919 with steam clouds is to be found on pp. 116—117 of the well-known French Weekly „Illustration" of Aug. 9th 1919, No. 3988. This picture was taken on May 22th 1919.

logist to the Netherland East Indian Mining Service Dr. G. L. L. KEMMERLING has not yet appeared. As far as I know the best map of the lahar of 1919 till now appeared in the „Keloet Number" of „Picturesque Netherlands East Indies", Vol. I, N°. 5 [1]). The scale

Fig. 1.

The hot lahars of the Kloet volcano, in 1919.

After G. L. L. KEMMERLING. Scale 1 : 400.000.

of this map is however not 1 : 250.000 as it states but nearly 1 : 400.000. The distribution of the lahar is based on data procured by KEMMERLING (fig. 1). From this map I computed the area occupied by the lahar to be 208 sq. km. (81 sq. miles) or in round numbers 200 sq. km.

[1]) Issued by the Official Tourist Bureau Weltevreden, Java. July 1919.

As regards the thickness of the lahar, for the present there are only few data at our disposal. In the upper course in the neighbourhood of the lahar Gupit the thickness is according to my estimate more than 50 m., but here the breadth is only about 200 m. This great thickness is connected with an accumulation behind the Durga-canyon (with a breadth of only 10—15 m.), through which the whole lahar of 1919 had to pass. This upper part of the lahar has now been strongly attacked by erosion, so that its thickness can be measured. More important for an estimate of the cubical capacity of the lahar is the thickness within the plain. KEMMERLING (lit. 14 p. 810) mentions in the town of Blitar a local thickness of 1,55—2,20 m. and observes that in the plain inundated by the mud flow all vegetation is covered under a layer of sand and stones to a depth of 40—60 cm.

If we assume an average thickness of the mudflow of 50 cm., the *cubical capacity of the lahar of* 1919 would amount to $0{,}5 \times 200$ million cubic m. $=$ **100** *million cu.m.*; if we assume the average thickness to be20 cm. only — and to assume a still lower average thickness would be quite absurd — then the cubical capacity would amount to $0{,}2 \times 200$ million cu.m. $=$ **40** *million cu.m.* The material of the lahar consists partly of ash, lapilli and bombs from the eruption of 1919 and in part of old lahar-material, brought up, eroded and swept away by the new mud flow. The cubical capacity of the crater lake amounts to 38 million cubic metres (lit. 14) [1]).

The proportion of solid materials (A) to water (W) in the hot lahar of 1919 consequently would have been as follows:

Supposing an average thickness of the lahar of 50 cm.

$$\frac{A}{W} = \frac{100}{38} = 2{,}6$$

Supposing an average thickness of the lahar of 20 cm.

$$\frac{A}{W} = \frac{40}{38} = \pm 1.$$

Now let us take up again the *Katmai* region.

It appears that GRIGGS has quite overlooked in his speculations that before the eruption of 1912 the Katmai volcano must have possessed a crater lake, just as is now the case according to his section

[1]) After the eruption of 1901 the water-mark of the crater lake was lowered artificially so that the cubical capacity was reduced from 44 million cu.m. (lit. 10) to 38 million cu.m. At present there is a tunnel under construction with a view to have the crater lake dry before the next eruption and in future to avoid the formation of hot lahars.

(lit. 2 p. 167), and the water of this crater lake must have acted a similar part as that of the Klut.

Thus the first sediment of the Katmai explosion would not have been as GRIGGS supposes the ash deposited as subaërial sediment but *the hot lahar*, which reduced the trees to charcoal. Afterwards there must have supervened a phase in which all the water of the crater was blown out and the ash was thrown up dry and fell down on an extensive area, and finally (as a phenomenon that is not connected directly with the eruption) the rain caused a cold mud flow on the South slope of Mt. Katmai.

Let us now trace the quantities of solid material and water.

GRIGGS estimates the quantity of solid material of the great mud flow of the Valley of ten thousand smokes at 4096 million cubic metres (lit. 5, p. 137) [1]) and the cubical capacity of the crater-hole after the eruption at 4500 million cubic yards = 3442 million cu.m. By the eruption the rim was lowered from 7500 feet (2280 m.) to a maximum of 6970 feet (2120 m.) (lit. 4, p. 167) or a minimum of 5200 feet (1580 m.) (lit. 3, p. 59). According to GRIGGS the crater-hole had before the eruption of 1912 a cubical capacity of 11,000 million cubic yards = 8415 million cu.m.

The content of the crater lake will therefore have amounted to 8415 million cu.m. before the eruption, so that the above mentioned proportion for the Katmai volcano would amount to:

$$\frac{A}{W} = \frac{4096}{8415} = 0{,}49.$$

If the cubic capacity of the crater lake before the eruption had been smaller and the quantity of solid material in the lahar greater, there would still have been enough water in the Katmai crater to cause the hot mud flow which was observed by GRIGGS in the Valley of ten thousand smokes.

The question whether the distribution of the mud-flow can be brought into harmony with the supposition that the Katmai crater was the source of the hot mud flow has still to be answered. GRIGGS gives on page 132 of his paper of Dec. 1918 (lit. 5), (fig. 2) a map with the distribution of the great hot mud flow. On the West side of Mt. Katmai the hot mud flow attains the contour line of 3000 feet (910 m.), while the lahar running in a northwest direction

[1]) In lit. 6, p. 241 GRIGGS computes the total quantity of the sand- (read : mud-flow) „as greater than a cubic mile".

over a distance of ± 15 miles (24 km.) descends to ± 300 feet (91 m.), (lit. 5, p. 120).

The Valley of ten thousand Smokes.
Het dal der tienduizend rookpluimen.
Simplified after R. F. GRIGGS.

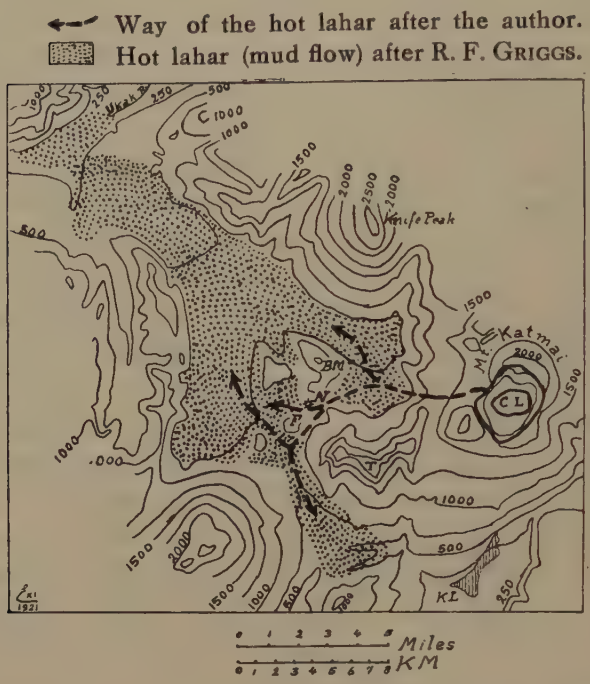

Fig. 2. Scale 1 : 400.000.
Heights in metres.

CL = Crater Lake.
KL = Katmai cold mudflow.
BM = Broken Mountain.
N = Novarupta volcano.
F = Falling Mountain.
T = Trident volcano.

Between *Broken Hill* and *Trident Volcano* lies the new volcano *Novarupta* which according to the terminology of SCHNEIDER (lit. 16, p. 67) must be classed with the *tholoïdes* [1]). The altitude of the pass between Novarupta and Trident Volcano amounts to 2600 feet

[1]) Novarupta is composed of a ring of loose efflata and a central lava plug (tholoïde) (lit. 6, fotograph p. 230). Just as at the Gunung Galunggung (Java) where in 1918 a tholoïde: Gunung Baru (= New Mountain) originated (lit. 18) in the crater, also at Novarupta the tholoïde is a volcanic structure in a crater, so that F. v. WOLFF (lit. 17, p. 491) seems to be right where he says that tholoïdes ought not to be included among the fundamental forms of volcanic structures. They are secondary forms in existing volcanoes.

(790 m.), while that of the pass between Falling Mountain and Trident Volcano is 2800 feet (850 M.) high.

The course which is taken in my opinion by the hot lahar is drawn on the annexed diagram (fig. 2) by a dashed line. Coming from Katmai crater with great rapidity the lahar descended towards the west, bifurcated the first time near Broken Hill, whereby the main part flowed westwards and a smaller part towards the south-west between Broken Hill and Trident Volcano. A second bifurcation took place near Falling Mt.; one part flowing in a westerly direction and after turning to the north joining the main flow, whereas the other part flowed to the south and southwest over Katmai-pass and descended as far as Mageik Creek to a level of ± 1400 feet (425 m.). This has been derived from GRIGGS' map. He will be able to say wether if it agrees with his detailed observations.

What still calls for an explanation is the fact that the mud flow has not been found between the western crater rim (± 6000 feet = 1820 m.) and the contour-line of 3000 feet (910 m.). A similar phenomenon, though less striking[1]) is also observed at the Klut. It is due to the fact that the lahar erodes in the shorter and steeper upper part of his course and can lay down deposits only within the much longer and more level lower part. The name Klut signifies: sweeper; at each eruption he entirely cleans away great parts of his slopes and further on sweeps away cultivations and villages.

In connection with the above mentioned phenomena I should like to propose the following nomenclature for mud flows:

I. *Specific volcanic mud flows*:

1. By an eruption through a crater lake: Type
   lahar (hot lahar) Klut (Java)
   (hot mud flow)
2. By melting of an ice-cap by an eruption: *jökulhlaup* Iceland

II. *Not specific vulcanic mud flows*:

3. By heavy rains on loose material: *Murgang* (cold lahar)
   (cold mud flow)

In the above I have tried to prove that the Katmai eruption of 1912 was an enlarged edition of the Klut eruptions which have been so often observed (fig. 3), and that the hot mud flow of the Valley of Ten Thousand Smokes was a hot lahar.

[1]) The narrow Durga-canyon brakes the lahar which therefore partially remains behind.

On the lahar of the Klut pseudo volcanic phenomena were observed in connection with evaporating rain- and groundwater and the sudden appearance of steam under great pressure. That this pressure can be rather high may be concluded from the temperature of 360° C. which KEMMERLING measured at a gas emanation from the lahar some days after the eruption.

The highest recorded temperature in the Valley of Ten Thousand Smokes was 645° C. (lit. 6 p. 250). I should not dare to assert that the smokes in the latter valley are coming all from the heat which is locked up in the lahar.

It is true that the material of the lahar is an excellent insulator against loss of heat, so that it seems quite possible that the lahar of Mt. Katmai may still retain a part of his own heat after years, but GRIGGS mentions three facts showing that there must be still another source of heat in the valley.

In the first place true solfatares were observed with the sublimation products sulphur and the two sulphides of arsenic (lit. 4 p. 105) which fact in accord with our experience must be brought back to magma the below.

Secondly, GRIGGS succeeded in finding fumaroles not situated on the mud flow but beside it in the Jurassic sandstone, which locally forms the underlying terrane of the valley (lit. 4 p. 111).

Thirdly at one end of the valley lava appeared and formed the lava-plug in Novarupta volcano (lit. 4 p. 111).

These three facts do not agree with the properties of the hot lahars from the Klut volcano and are explained by GRIGGS on the supposition that under the valley the magma lies near the surface.

On the other hand it is certain that the Katmai lahar must form pseudo-volcanic phenomena just as the Klut lahar did; so it will probably be possible to conclude from detailed data whether we must indeed suppose (lit. 4 p. 116) that the area in the vicinity of which the magma reaches nearly to the surface has a length of 32 Km. or that parts of this terrane do not exhibit signs of real volcanic phenomena, but only of pseudo-volcanic lahar phenomena.

Probably this might be settled by help of analyses of the fumarole gases and a map of the distribution of the different kinds of gas.

It seems conceivable for instance that the part of the Katmai lahar which flowed over Katmai-pass towards the southeast does not have real solfatares. In his last paper (lit. 6 p. 248) GRIGGS points to the fact that the intensity of some fumaroles was in 1919 lower than in the preceding years. Some fumaroles had in 1919 a markedly lower temperature, others (about a hundred) did not exist any longer.

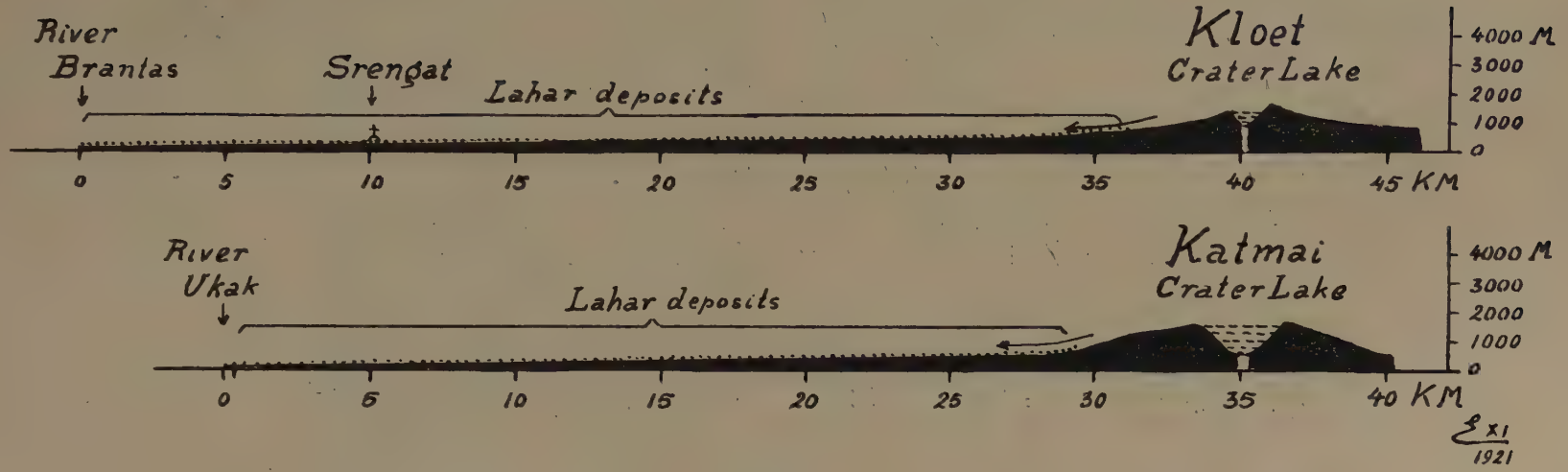

Fig. 3. Scale 1 : 250.000.

Two hot Lahar's of

Katmai (Alaska) after the eruption of 1912
and *Klut* (Java) after the eruption of 1919.

It might be possible that these observations have relation to pseudo-fumaroles.

In the discussion of the great mud flow I have till now used only the papers of GRIGGS litt. 1—5. Especially in lit. 5 he gives many detailed observations on the mudflow. It may be noted that he speaks of a "high-water-mark" (lit. 5 p. 121) and in the legend accompanying a photograph (lit. 5 p. 126) of a "high-mud-mark".

In lit. 6 however a "high-sand-mark" (p. 232) is for the first time mentioned and it is evident that GRIGGS interprets the tuff-filling of the valley by the eruption of *dry* ash and sand through hundreds of little volcanoes in the bottom of the valley. He speaks of "the tremendous outflow of incandescent sand" (p. 241) and in the legend to a photograph (p. 228) we find the expression "hot-sand-flow".

The mechanism of this hot sand-flow is for the present insufficiently explained: „The continuity of this „high-sand-mark" shows clearly that the incandescent mass was not poured down one of the adjacent mountain sides into the valley, but must have originated from vents within its confines. This is clearly evidenced by many additional facts which cannot be detailed here. During the whole period of flow the mass was probably kept in a state of constant turmoil by the continued evolution of gas from the substance of its solid components" (lit. 6 p. 232).

In connection with the photograph of Novarupta (lit. 6 p. 230) we find mentioned: "It is probable that a considerable fraction of the incandescent sand came from this vent."

This new hypothesis of GRIGGS is manifestly invented because on a closer view he no longer found it probable that a hot mud flow welled out of the bottom of the valley.

However this may be, *his detailed observations communicated in lit. 5, have convinced me that the tuff-filling of the „Valley of ten thousand smokes" is brought about by a hot lahar coming from the crater lake of Mt. Katmai.*

We can therefore predict with a great approach to certainty that at the next eruption of Katmai-volcano hot lahars will again occur and principally on that side or those sides where the crater-rim is now lowest.

Nov. 21, 1921.

REFERENCES.

I. Mt. Katmai.

1. Robert F. Griggs: „*The Valley of ten thousand smokes*". The National Geographic Magazine. Washington 1917, January pp. 13—68.

2. R. F. Griggs: „*The Valley of ten thousand smokes*". Washington 1918, February pp. 115—169.

3. R. F. Griggs: „*The Recovery of Vegetation at Kodiak*". The Ohio Journal of Science 1918. November pp. 1—57.

4. R. F. Griggs: „*Are the ten thousand smokes real volcanoes?*" The Ohio Journal of Science 1918. December pp. 97—116.

5. R. F. Griggs: „*The great hot mud flow of the valley of ten thousana smokes*". The Ohio Journal of Science 1918. December pp. 117—142.

6. R. F. Griggs: „*Our greatest national monument*". The National Geographic Magazine 1921. September p.p. 219—292.

II. Kloet.

7. Fr. Junghuhn: „*Java*". Sec. edition 1854. Part III, pp. 668—723.

8. R. D. M. Verbeek et R. Fennema: „*Description géologique de Java et Madoura*". 1899. Tome I, pp. 177—178.

9. L. Houwink: „*Verslag van een onderzoek naar aanleiding van de uitbarsting van den vulkaan Keloet in den nacht van den 22en op den 23en Mei 1901*". Jaarboek v. h. Mijnwezen in Ned. Oost-Indië. 35e jaarg. 1901, pp. 122—136.

10. H. Cool: „*Eenige mededeelingen en beschouwingen naar aanleiding van een onderzoek aan den kraterwand van den Keloet in Mei 1907*". Jaarboek v. h. Mijnwezen in Ned. Oost-Indië. 1907, pp. 185—233.
(With a list of older references on the Klut-eruptions).

11. G. Vissering: „*Geweldige Natuurkrachten*". Batavia-Amsterdam 1910.

12. B. G. Escher: „*De Kloet van een geomorfologisch standpunt beschouwd*". Natuurkundig Tijdschrift voor Ned. Indië. Vol. LXXIX, pp. 120—127. 1919.

13. B. G. Escher: „*De Kloet*". Waterstaats-Ingenieur. 7e jaargang, pp. 304—309. 1919.

14. G. L. L. Kemmerling: „*De Kloetramp*". De Ingenieur. 34e jaargang, pp. 804—813. 1919.

III. Miscellaneous.

16. K. Schneider: „*Die vulkanischen Erscheinungen der Erde*".

17. F. von Wolff: „*Der Vulkanismus*".

18. Th. Thoroddsen: „*Island*". Ergänzungshefte zu Petermann's Mitteilungen No. 152—153.

19. B. G. Escher: „*L'éruption du Gounoung Galounggoung en Juillet 1918*". Natuurkundig Tijdschrift v. Ned. Indië. Vol. LXXX, pp. 260—264. 1920.

Physics. — "*On the Equation of State for Arbitrary Temperatures and Volumes.* III. *On the Law of Force between the Molecules of Mon-atomic Substances*". By Dr. J. J. VAN LAAR. (Communicated by Prof. H. A. LORENTZ).

(Communicated at the meeting of November 26, 1921).

§ 8. General Considerations.

In the two preceding papers [1]) it has been demonstrated that a closer consideration of the problem of the movement of a molecule to and fro between the two adjacent molecules (for the sake of simplicity reduced to a problem of one-dimension) necessarily leads at *low* temperatures to an expression of the form

$$\varepsilon = \Delta + \frac{2\Delta}{e^{\frac{2/3\,\Delta}{RT}} - 1},$$

in which Δ represents the *zero-point energy*, i.e. the energy of the active forces, which remains when the temperature (determined by the time-average of u^2) has become $= 0$ [loc. cit. p. 1198 (Δ is there represented by E_0) and p. 905].

If it could be proved that in this $\Delta = {}^3/_2 Nh\nu$, the analogy with PLANCK's formula would become identity. But to reach this, we should have to know the accurate law of attraction, i.e. a law which takes into account the *motion in closed orbits* of the negative electrons round the positive nucleus of the atoms. The prevalent laws of attraction have not taken this into account as yet; either because the integrability of the equations of motion required a simple — although still plausible — law of attraction, so that the accurate law had to be purposely set aside for one of a simpler form; or because in the derivation of the required law the influence of the said motion was (consciously or unconsciously) eliminated

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[1]) These Proc. Vol. XXI, N°. 9, p. 1184, and These Proc. Vol. XXIII, N°. 6, p. 887.

by taking averages, as e.g. DEBIJE did in his paper on the VAN DER WAALS' cohesion forces [1]).

In our first paper (see p. 1188) we assumed for the attractive force $F = f \times 2x$, in which x represents the distance of the moving molecule from the neutral point; and for the repulsive force on collision $2\varepsilon \times y$, in which y represents the compression of the molecule.

Later on (p. 897) I substituted for the two separate laws of force one single law of the form (cf. the cited paper for the meaning of the different letters)

$$F = f \,.\, 2x \left[1 - \frac{(\varrho - s)(\sigma - s)}{(l-s)^2 - x^2} \right],$$

which rendered the solution of the problem raised there still just possible by the aid of elliptical functions.

Though these two laws of attraction by no means represent reality quantitatively accurately, yet at low temperatures we found a relation between E and T, which is analogous with PLANCK's well-known expression — which certainly proves that the essential part of our considerations (viz. our observance of the *time-average*) rests on solid foundations. The exact form of the law of attraction seems here to a certain extent to be of minor importance, and according to the results of the two papers to have influence only on some numerical coefficients.

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[1]) Phys. Zeitschr. 21, 178 (1920). In 1908 VAN DER WAALS Jr. already treated a simular problem, but he still considered the atoms (molecules) as electric double points which, like DEBIJE, he besides supposed *far enough* apart to simplify the problem. He found that the force decreased more rapidly than r^{-7}.

Later also KEESOM wrote a paper in connection with the said paper by DEBIJE (These Proc. Vol. XXIII, N°. 6, p. 939 and 943; also Phys. Zeitschr. 22, 129 (1921) and Mededeelingen Utrecht N°. 6) on the question of the forces of attraction. There he demonstrates that for H_2, O_2 and N_2 DEBIJE's quadrupoles yield a too large value; further that — at least for the said gases — the quadrupole-attraction has considerably more influence on the second virial-coefficient B than the so-called "induced" attraction (unless the temperature is very high), and that the VAN DER WAALS cohesion-forces can chiefly be attributed to forces which the molecules exert on each other in virtue of their quadrupole-momenta.

BURGERS (Dissertatie, Leiden 1918, p. 186) calculated the quadrupole-momentum of the H_2-molecule according to the model constructed by BOHR and DEBIJE (to which, however, there are several objections), and found a remarkable agreement with the value $2{,}03.10^{-26}$ (electrostatic units $\times$ cm^2.; uncorrected for the polarisation of the molecules in each others' electric field), derived by KEESOM (Comm. Leiden, Suppl. 39*a*, p. 15) from the 2nd virial-coefficient, viz. $2{,}05.10^{-26}$.

But, as has been said, a true insight — especially as far as the relation between Δ and $h\nu$ is concerned — cannot be obtained until the law of attraction is accurately known; then the quantities h and ν will also appear automatously in the law of the action between two atoms (molecules).

We shall see in what follows that the real active force is by no means merely an exclusively *attractive* action increasing in intensity, which is only transformed into a *repulsive* force at collision or at a very short distance — but that from the very beginning the action has been **periodically** attractive and repulsive, which only becomes stronger and stronger on approach of the atoms. Several known phenomena can now be explained more easily, not only in the sphere of the solid bodies (and of the liquids), but also in that of the *gases* — particularly as concerns the so-called "*gas-degeneration*" at very low temperatures.

§ 9. Derivation of the Elementary Law of Force.

To simplify the calculations, we shall again place ourselves at the standpoint of the problem of one dimension. In connection with this the circular motion of the electrons round the positive nucleus must be transformed into a motion to and fro rectilinearly, viz. the projection of the circular motion on the direction of the joining line of the two nuclei, so that the electrons always move (fictitiously) to and fro through the nucleus.

Let r be the distance of the two nuclei A and B, a the radius of the orbit of the electrons (thought perfectly circular), so that the deviations x and y of the electrons from the centre are represented by $x = a \sin 2\pi \frac{t}{T}$, $y = a \sin 2\pi \frac{t'}{T}$. We then have together for the repulsive and attractive forces (see Fig. 1):

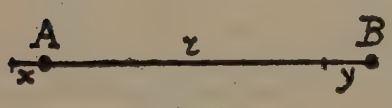

Fig. 1.

$$F = e^2 \left[-\frac{1}{r^2} - \frac{1}{(r+x-y)^2} + \frac{1}{(r+x)^2} + \frac{1}{(r-y)^2} \right],$$

when for the present we confine ourselves in our considerations to mon-atomic substances, while only *one* electron moves round the nucleus. (H-atoms).

When an atom M moves between two other atoms P and Q, the total action (taken positive when M is drawn to the right (P))

becomes evidently (see fig. 2) — on the supposition that the atoms

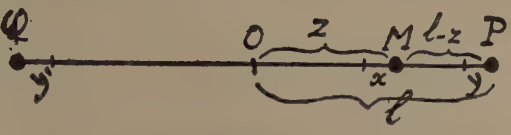

Fig. 2.

P and Q are on an average at rest, and the mutual polarizing action of the atoms may be neglected:

$$\frac{F}{e^2} = -\left\{\frac{1}{(l-z)^2} - \frac{1}{(l-z+x-y)^2} + \frac{1}{(l-z+x)^2} + \frac{1}{(l-z-y)^2} + \frac{1}{(l+z)^2} + \frac{1}{(l+z-x-y')^2} - \frac{1}{(l+z-x)^2} - \frac{1}{(l+z-y')^2}\right\},$$

when l is the mean distance of the atoms, and z the distance of the moving atom on the right from the mean position of equilibrium (neutral point) O. If therefore F is positive, the force of M is directed towards P.

Now the motion of the electrons round P and Q will exhibit *phase-difference* with that of the electron round M, so that we shall have to calculate the mean value for different values of y and y', *retaining* the value of x, that varies periodically with the time in the *considered* molecule M, which we shall, accordingly, *not* eliminate by taking averages. The integral

$$I = \frac{1}{2\pi}\int_0^{2\pi} \frac{d\omega}{((l-z)+x-y)^2} = \frac{1}{2\pi}\int_0^{2\pi} \frac{d\omega}{\left[(l-z)+a\sin 2\pi\frac{t}{T} - a\sin\left(2\pi\frac{t}{T}+\omega\right)\right]^2}$$

has the form

$$I = \frac{1}{2\pi}\int_{\varphi}^{\varphi+2\pi} \frac{d\varphi}{(p-a\sin\varphi)^2},$$

when $2\pi\frac{t}{T} + \omega = \varphi$ is put. (ω is the phase-difference between P and M). With $\alpha = 90 + \varphi$ this becomes:

$$I = \frac{1}{2\pi}\int_{\alpha}^{\alpha+2\pi} \frac{d\alpha}{(p+a\cos\alpha)^2} = \frac{p}{(p^2-a^2)^{3/2}},$$

as is easy to derive. Hence we get, performing the same thing with the integrals in which y' occurs:

$$\frac{F}{e^2} = \left.\begin{aligned} &-\frac{1}{(l-z)^2} - \frac{l-z+x}{((l-z+x)^2-a^2)^{3/2}} + \frac{1}{(l-z+x)^2} + \frac{l-z}{((l-z)^2-a^2)^{3/2}} \\ &+\frac{1}{(l+z)^2} + \frac{l+z-x}{((l+z-x)^2-a^2)^{3/2}} - \frac{1}{(l+z-x)^2} - \frac{l+z}{((l+z)^2-a^2)^{3/2}} \end{aligned}\right\}, \quad (1)$$

and this will be the law of action between M and the two adjacent atoms P and Q. The first four terms refer to P, towards which M e.g. moves; the last four to Q, from which M then moves away.

The expression (1) yields in the equation of motion perfectly unintegrable forms, and we shall try to find for them an approximate expression, when z aud x are not too great with respect to l. At any rate it appears at once that F *contains the factor* x, so that the law of action becomes a **purely periodical** one.

Whenever x becomes $= 0$, i.e. the electron (fictitiously) moves through the nucleus, F will also become $= 0$, and the total force change from *positive* (at x positive), i.e. directed towards the *right*, into negative (at x negative), i.e. directed towards the *left*, and vice versa. In reality for $x = 0$ both the first part ot the second member of (1) becomes $= 0$, and the second part.

When in his cited article (p. 179 righthand side) DEBIJE states that the potential of a sphere with charge $+ e$ in its centre and $- e$ on its circumference is on an *average* $= 0$, he is, of course, right. But in the first place I object to this view of the problem, since it will depend on the mutual position of the electrons in their orbits round the two centres and on their phace-difference, what action will result; which also renders it doubtful whether all orientations of the two electrons on the two sphere-surfaces will, indeed, be equivalent — even on an "average". And in the second place it seems to me that his method — in order to find still a positive value (i.e. attractive action) for the resulting force — of taking the action into account which one atom exerts on the electric moment of the other, is open to doubt. For according to DEBIJE himself the electric field of this one atom will be on an average $= 0$ (see a few lines lower). How can then this field, which is $= 0$ on an average, exert an appreciable polarizing action[1]) on the other atom?

That the attractive action with the periodicity found by us, is

[1]) Even apart from the fact that the polarization will certainly always be very small, because in my opinion the exceedingly great velocity of the electrons in their orbits excludes an appreciable deformation. DEBIJE finds finally for the attractive action proportionality as r^{-9} (for so-called dipole gases on the other hand r^{-7}, cf. note 1 on page 183), as against VAN DER WAALS Jr. as $r^{-(7+\delta)}$.

not quite symmetrical with the repulsive action, is clear from formula (1). More-over, also for an estimation of the relative order of magnitude of these forces, we shall give some numerical considerations in the following paragraph.

§ 10. Some Numerical Calculations.

Since the centres of the atoms cannot get nearer to each other than $2a$, a mean distance of $l = 3a$ is, indeed, an extreme value for solid bodies and liquids, sooner too great than too small. For when it is considered that for many liquids $(v-b):v$ is $= {}^1/_{14}$ in the neighbourhood of the point of solidification, then $l = (1 + {}^1/_{42})\,2a$. But as the quantity b in the equation of state will very certainly not be equal to the real volume of the molecules, but larger, in reality l will be $> 2{,}05\,a$. Even at the absolute zero-point l will probably not be smaller than $2{,}1\,a$. Let us now first put

$$l = 3a.$$

1. $z = 0$. The moving molecule is then exactly in O, halfway between the two others.

We can now write for (1):

$$\frac{F}{e^2} = \left[\left\{\frac{l-z}{((l-z)-a^2)^{3/2}} - \frac{1}{(l-z)^2}\right\} - \left\{\frac{l-z+x}{((l-z+x)^2-a^2)^{3/2}} - \frac{1}{(l-z+x)^2}\right\}\right] - \left\{\text{id. with } +z \text{ and } -x\right\}, \quad (1a)$$

in which we get for the case $z = 0$:

$$\frac{F}{e^2} = \left[\left\{\frac{l}{(l^2-a^2)^{3/2}} - \frac{1}{l^2}\right\} - \left\{\frac{l+x}{((l+x)^2-a^2)^{3/2}} - \frac{1}{(l+x)^2}\right\}\right] - \left[\left\{\frac{l}{(l^2-a^2)^{3/2}} - \frac{1}{l^2}\right\} - \left\{\frac{l-x}{((l-x)^2-a^2)^{3/2}} - \frac{1}{(l-x)^2}\right\}\right],$$

in which the first and the third part cancel each other. In order, however, to get to know something of the mutual order of magnitude of the different parts, we have not omitted these terms.

For $x = 0$ all the 4 terms are equal to each other; i.e. $= (3 : 8^{3/2} - 1 : 9) : a^3 = (0{,}1326 - 0{,}1111) : a^3 = 0{,}0215 : a^3$, and we have

$$a^3 F : e^2 = (0{,}0215 - 0{,}0215) - (0{,}0215 - 0{,}0215) = 0 - 0 = 0.$$

For $x = +a$ (extreme deviation of the electron towards the side of Q (see fig. 2) is found with $4 : 15^{3/2} - (1 : 16) = 0{,}0689 - 0{,}0625 = 0{,}0064$, and $2 : 3^{3/2} - 1 : 4 = 0{,}3849 - 0{,}25 = 0{,}1349$:

$$a^3 F : e^2 = (0{,}0215 - 0{,}0064) - (0{,}0215 - 0{,}1349) = 0{,}0151 - (-0{,}1134) = 0{,}1285.$$

A force, therefore, directed towards the right, chiefly originating from the repulsive action exerted by Q on M.

For $x = -a$ (greatest deviation to the side of P) everything is just the opposite, and we have:

$$a^2 F : e^2 = (-0{,}1134) - 0{,}0151 = -0{,}1285.$$

Now the repulsive force predominates, which P exerts on M.

2. $z = {}^1/_2\, a$. This is a mean position of M between O and the perfect contact at $z = a$.

For $x = 0$ we have here, since $2{}^1/_2 : (5{}^1/_4)^{3/2} - (1 : 6{}^1/_4) = 0{,}2078 - 0{,}16 = 0{,}0478$, and $3{}^1/_2 : (11{}^1/_4)^{3/2} - (1 : 12{}^1/_4) = 0{,}09276 - 0{,}08163 = 0{,}0111$:

$$a^2 F : e^2 = (0{,}0478 - 0{,}0478) - (0{,}0111 - 0{,}0111) = 0 - 0 = 0.$$

For $x = a$ we find:

$$a^2 F : e^2 = (0{,}0478 - 0{,}0111) - (0{,}0111 - 0{,}0478) = 0{,}0366 - (-0{,}0366) = 0{,}0732.$$

Here the attractive force of P supports the repulsive force of Q (which happens to have the same value).

Finally $x = -a$ yields with $1{}^1/_2 : (1{}^1/_4)^{3/2} - (1 : 2{}^1/_4) = 1{,}0733 - 0{,}4444 = 0{,}6289$, and $4{}^1/_2 : (19{}^1/_4)^{3/2} - (1 : 20{}^1/_4) = 0{,}0533 - 0{,}0494 = 0{,}0039$:

$$a^2 F : e^2 = (0{,}0478 - 0{,}6289) - (0{,}0111 - 0{,}0039) = (-0{,}5811) - 0{,}0072 = -0{,}5883.$$

It will be seen that the action is now quite asymmetric: that towards the right at $x = +a$ is much weaker than that towards the left at $x = -a$. This is, of course, owing to the fact that in this latter position the electron is much nearer P than it is to Q in the case $x = +a$.

3. $z = a$. This is the extreme position of M close to P (distance of the centres $= 2a$), in which we shall now find an infinitely great repulsive force at $x = -a$.

In the case $x = 0$ we get:

$$a^2 F : e^2 = (0{,}1349 - 0{,}1349) - (0{,}0064 - 0{,}0064) = 0 - 0 = 0.$$

For $x = +a$ we find:

$$a^2 F : e^2 = (0{,}1349 - 0{,}0215) - (0{,}0064 - 0{,}0215) = 0{,}1134 - (-0{,}0151) = 0{,}1285.$$

And $x = -a$ yields with $(1 : 0^{3/2}) - (1 : 1) = \infty - 1$, and $(5 : 24^{3/2}) - (1 : 25) = 0{,}0425 - 0{,}04 = 0{,}0025$:

$$a^2 F : e^2 = (0{,}1349 - \infty) - (0{,}0064 - 0{,}0025) = (-\infty) - 0{,}0039 = -\infty.$$

The above can be combined in the following survey (values of $a^2 F : e^2$).

		$z=0$	$z=\frac{1}{2}a$	$z=a$
	$x=0$	0	0	0
$(l=3a)$	$x=+a$	0,1285	0,0732	0,1285
	$x=-a$	−0,1285	−0,5883	$-\infty$

Let us now repeat these calculations for the case

$$l=2{,}1a$$

1. $z=0$.

For $x=+a$ is found with $3{,}1:(8{,}61)^{3/2}-(1:9{,}61)=0{,}12-$ $-0{,}10=0{,}02$, and $1{,}1:(0{,}21)^{3/2}-(1:1{,}21)=11{,}43-0{,}83=10{,}60$:

$$a^3F:e^3=-0{,}02+10{,}60=10{,}58;$$

while for $x=-a$ of course $-10{,}58$ will be found.

2. $z=0{,}05a$.

With $2{,}05:(3{,}2025)^{3/2}-(1:4{,}2025)=0{,}36-0{,}24=0{,}12$; $2{,}15:$ $:(3{,}6225)^{3/2}-(1:4{,}6225)=0{,}31^3-0{,}21^6=0{,}10$; $3{,}05:(8{,}3025)^{3/2}-$ $-(1:9{,}3025)=0{,}13-0{,}11=0{,}02$; $1{,}15:(0{,}3225)^{3/2}-(1:1{,}3225)=$ $=6{,}28-0{,}76=5{,}52$ we get for $x=+a$:

$$a^3F:e^3=(0{,}12-0{,}02)-(0{,}10-5{,}52)=5{,}52,$$

while $x=-a$, with $3{,}15:(8{,}9225)^{3/2}-(1:9{,}9225)=0{,}12-0{,}10=$ $=0{,}02$; $1{,}05:(0{,}1025)^{3/2}-(1:1{,}1025)=32{,}00-0{,}91=31{,}09$, gives:

$$a^3F:e^3=(0{,}12-31{,}09)-(0{,}10-0{,}02)=-31{,}05.$$

3. $z=1{,}1a$.

With $(2:3^{3/2})-(1:4)=0{,}38^5-0{,}25=0{,}13^5$; $2{,}2:(3{,}84)^{3/2}-(1:$ $:4{,}84)=0{,}29^{24}-0{,}20^{66}=0{,}08^6$; $(3:8^{3/2})-(1:9)=0{,}13-0{,}11=$ $=0{,}02$; $(1{,}2:0{,}44^{3/2})-(1:1{,}44)=4{,}11-0{,}69=3{,}42$ we get for $x=+a$:

$$a^3F:e^3=(0{,}13^5-0{,}02)-(0{,}08^6-3{,}42)=3{,}45.$$

The value $-\infty$ will again be found for $x=-a$. Hence we have now for $a^3F:e^3$:

		$z=0$	$z=0{,}05a$	$z=0{,}1a$
	$x=0$	0	0	0
$(l=2{,}1a)$	$x=+a$	10,58	5,52	3,45
	$x=-a$	−10,58	−31,05	$-\infty$

In consequence of the so much smaller distance of the molecules, the action has in many cases become as much as 80-times greater.

It appears from the above calculations that at *low* temperatures, in which case l approaches to $2a$, the terms without x can very well be omitted, so that we then might write, putting z everywhere $= 0$:

$$\left(\begin{matrix}\text{low temp.}\\ l \text{ — } 2a\end{matrix}\right)\frac{F}{e^2}=-\left[\frac{l+x}{((l+x)^2-a^2)^{3/2}}-\frac{1}{(l+x)^2}\right]+\left[\frac{l-x}{((l-x)^2-a^2)^{3/2}}-\frac{1}{(l+x)^2}\right], \quad (1b)$$

in which the 1st term predominates at $x = -a$, the 2nd at $x = +a$.

Though we may now write:

$$\frac{d^2z}{dt^2}=\frac{e^2}{m}\left[\frac{d}{dx}\left\{\frac{1}{\sqrt{(l+x)^2-a^2}}-\frac{1}{l+x}\right\}+\frac{d}{dx}\left\{\frac{1}{\sqrt{(l-x)^2-a^2}}-\frac{1}{l-x}\right\}\right]=\frac{e^2}{m}\frac{dL}{dx},$$

the direct integration of this equation is impossible. For, since $x = a \sin\varphi$ (in which φ is in general $= 2\pi\frac{t}{T'}+\theta$, because at the beginning of the motion of M through O towards $P(t=0$, when M in $O)$ the electron need not necessarily at the same time be in the position $x=0)$, we get:

$$\frac{d^2z}{dt^2}=\frac{e^2}{m}\frac{dL}{dt}\frac{dt}{dx}=\frac{e^2}{m}\frac{dL}{dt}\frac{T:2\pi}{a\cos(2\pi t/T+\theta)}=\frac{c}{a\cos\varphi}\frac{dL}{dt},$$

and evidently nothing can be done with this differential equation — on account of the complicated form of L, while $\sqrt{a^2-x^2}$ can be substituted for $a\cos\varphi$.

Hence nothing remains but making the expression (1^b) or (1^a) integrable by expansion into series.

§ 11. Expansion into Series for F.

Instead of e.g. $\frac{l-z}{((l-z)^2-a^2)^{3/2}}-\frac{1}{(l-z)^2}$ we shall expand

$$\frac{1}{\sqrt{(l-z)^2-a^2}}-\frac{1}{l-z},$$

and then differentiate the result with respect to z, which is easier. We then get:

$$\frac{1}{(l-z+a)^2(l-z-a)^{1/2}}-\frac{1}{l-z}=\frac{1}{\sqrt{l^2-a^2}}\left\{\left(1-\frac{z}{l+a}\right)^{-1/2}\left(1-\frac{z}{l-a}\right)^{-1/2}\right\}-$$

$$-\frac{1}{l}\left(1-\frac{z}{l}\right)^{-1}.$$

For the expression between {} we may write by joining corresponding terms:

$$1+\frac{1}{2}2z\frac{l}{l^2-a^2}+\frac{1.3}{2.4}2z^2\frac{l^2+a^2}{(l^2-a^2)^2}+\frac{1.3.5}{2.4.6}2z^3\frac{l^3+3la^2}{(l^2-a^2)^3}+$$

$$+\frac{1.3.5.7}{2.4.6.8}2z^4\frac{l^4+6l^2a^2+a^4}{(l^2-a^2)^4}+\text{etc.}$$

$$+\frac{1}{2}\cdot\frac{1}{2}z^2\frac{1}{l^2-a^2}+\frac{1}{2}\cdot\frac{1.3}{2.4}2z^3\frac{l}{(l^2-a^2)^2}+\frac{1}{2}\cdot\frac{1.3.5}{2.4.6}2z^4\frac{l^2+a^2}{(l^2-a^2)^3}+\text{etc.}$$

$$+\frac{1.3}{2.4}\cdot\frac{1.3}{2.4}z^4\frac{1}{(l^2-a^2)^2}+\text{etc.},$$

the structure of which is clear. Hence, after multiplication by $(l^2-a^2)^{-1/2}$ and expansion of $\left(1-\frac{z}{l}\right)^{-1}$, the original form becomes:

$$\left(\frac{1}{\sqrt{l^2-a^2}}-\frac{1}{l}\right)+z\left(\frac{\frac{1}{2}2l}{(l^2-a^2)^{3/2}}-\frac{1}{l^2}\right)+z^2\left(\frac{\frac{1.3}{2.4}2(l^2+a^2)+\frac{1}{2}\cdot\frac{1}{2}(l^2-a^2)}{(l^2-a^2)^{5/2}}-\frac{1}{l^3}\right)+$$

$$+z^3\left(\frac{\frac{1.3.5}{2.4.6}2(l^3+3la^2)+\frac{1}{2}\cdot\frac{1.3}{2.4}2l(l^2-a^2)}{(l^2-a^2)^{7/2}}-\frac{1}{l^4}\right)+$$

$$+z^4\left(\frac{\frac{1.3.5.7}{2.4.6.8}2(l^4+6l^2a^2+a^4)+\frac{1}{2}\cdot\frac{1.3.5}{2.4.6}2(l^2+a^2)(l^2-a^2)+\frac{1.3}{2.4}\cdot\frac{1.3}{2.4}(l^2-a^2)^2}{(l^2-a^2)^{9/2}}-\frac{1}{l^5}\right)+\text{etc.},$$

i.e.

$$A+P_0z+P_1z^2+P_2z^3+P_3z^4+\ldots,$$

in which A, P_0, etc. contain only l and a. Through differentiation now arises:

$$\frac{l-z}{((l-z)^2-a^2)^{3/2}}-\frac{1}{(l-z)^2}=P_0+2P_1z+3P_2z^2+4P_3z^3+\ldots,$$

and likewise:

$$\frac{l+z}{((l+z)^2-a^2)^{3/2}}-\frac{1}{(l+z)^2}=P_0-2P_1z+3P_2z^2+4P_3z^3+\ldots,$$

so that the difference of these two expressions (c.f. equation (1a)) is represented by

$$f(z)=4P_1z+8P_3z^3+12P_5z^5+\ldots$$

After substitution of $z-x$ for z, we have also:

$$f(z-x)=4P_1(z-x)+8P_3(z-x)^3+12P_5(z-x)^5+\ldots,$$

and finally the following equation is obtained:

$$F:e^2=f(z)-f(z-x)=4P_1x+8P_3(3z^2x-3zx^2+x^3)+$$

$$+12P_5(5z^4x-10z^3x^2+\ldots+x^5)+\text{etc.},\quad\ldots\quad(2)$$

so that F clearly contains the factor x (see § 9).

After calculation of the numerators (to which also P_5 has been added), we have:

$$\left.\begin{aligned} P_1 &= \frac{l^2+{}^1/_3 a^2}{(l^2-a^2)^{5/2}} - \frac{1}{l^3} \quad ; \quad P_3 = \frac{l^2+3l^2a^2+{}^3/_5 a^4}{(l^2-a^2)^{9/2}} - \frac{1}{l^5} \\ P_5 &= \frac{l^6+{}^{15}/_3 l^4a^2+{}^{45}/_8 l^2a^4+{}^5/_{16}a^6}{(l^2-a^2)^{13/2}} - \frac{1}{l^7} \quad ; \quad \text{etc.} \end{aligned}\right\} \quad . \quad (3)$$

For small values of z $F : e^2$ approaches:

$$(z=0) \quad \underline{F : e^2 = 4P_1 x + 8P_3 x^3 + 12P_5 x^5 + \text{ etc.}} \; ; \; \underline{x = a\left(\sin\frac{2\pi t}{T} + \theta\right)} \quad (4)$$

Remark. That the coefficients of l^2, l^4, l^6, etc. in the expressions for P_1, P_3, P_5, etc. become every time $=1$, is not surprising. For they are resp. $2\left(\frac{1.3}{2\,4}\right)+\left(\frac{1}{2}\right)^2$, $2\left(\frac{1.3.5.7}{2.4.6.8}\right)+2\left(\frac{1}{2}\cdot\frac{1.3.5}{2\,4\,6}\right)+\left(\frac{1.3}{2.4}\right)^2$, etc., being the coefficients of the expansion of $(1-y)^{-1/2}(1-y)^{-1/2}$, i.e. of $(1-y)^{-1}$, which are all $=1$. The coefficients of a^2, a^4, a^6 etc., viz. ${}^1/_3$, ${}^3/_8$, ${}^5/_{16}$, etc. are evidently those of the expansion $(l=0)$ of $\left(1-\frac{z}{a}\right)^{-1/2}\left(1+\frac{z}{a}\right)^{-1/2}=\left(1-\frac{z^2}{a^2}\right)^{-1/2}$, i.e. $\frac{1}{2}$, $\frac{1.3}{2.4}$, $\frac{1.3.5}{2.4.6}$, etc. According to what follows, the coefficients of the second terms, viz. ${}^1/_3$, 3, ${}^{15}/_3$, 14, etc. are represented for $P_1, P_3 \ldots . P_n$ by ${}^1/_4\, n\,(n+1)$, i.e. by $\frac{1\times2}{4}$, $\frac{3\times4}{4}$, $\frac{7\times8}{4}$, etc. [When we add to this resp. ${}^5/_3$, ${}^9/_3$, etc. $(={}^1/_3\,(2n+3))$ of the exponents in the denominators, we get the coefficients of the limiting values of P_1, P_3, etc. for $l=\infty$, mentioned farther on viz. ${}^1/_4\,(n+2)\,(n+3)=\frac{3\times4}{4}$, $\frac{5\times6}{4}$, $\frac{7\times8}{4}$, etc.].

Indeed, a somewhat different way of expansion into series of $\frac{l-z}{((l-z)^2-a^2)^{3/2}} - \frac{1}{(l-z)^2}$ leads to

$$\frac{1}{(l-z)^2}\left[\frac{3}{2}\frac{a^2}{(l-z)^2}+\frac{3.5}{2.4}\frac{a^4}{(l-z)^4}+\cdots\right]=\frac{3}{2}\frac{a^2}{(l-z)^4}+\frac{3.5}{2.4}\frac{a^4}{(l-z)^6}+\cdots=$$

$$=\frac{3}{2}\frac{a^2}{l^4}\left(1-\frac{4}{1}\frac{z}{l}+\frac{4.5}{1.2}\frac{z^2}{l^2}+\cdots\right)+\frac{3.5}{2.4}\frac{a^4}{l^6}\left(1+\frac{6}{1}\frac{z}{l}+\frac{6.7}{1.3}\frac{z^2}{l^2}+\cdots\right)+\text{etc.}=$$

$$=\left({}^3/_2\frac{a^2}{l^4}+\frac{3.5}{2.4}\frac{a^4}{l^6}+\cdots\right)+z\left(\frac{3}{2}\cdot\frac{4}{1}\frac{a^2}{l^5}+\frac{3.5}{2.4}\cdot\frac{6}{1}\frac{a^4}{l^7}+\cdots\right)+$$

$$+z^2\left(\frac{3}{2}\cdot\frac{4.5}{1.2}\frac{a^2}{l^6}+\frac{3.5}{2.4}\cdot\frac{6.7}{1.2}\frac{a^4}{l^8}+\cdots\right)+\text{etc.},$$

so that for the factor of z is found (see above):

$2P_1 = \frac{a^2}{l^5}\left(\frac{3}{2}\cdot\frac{4}{1}+\frac{3\,5}{2.4}\cdot\frac{6}{1}\frac{a^2}{l^2}+\ldots\right)$; for that of z^3 we have

$4P_3 = \frac{a^2}{l^7}\left(\frac{3}{2}\cdot\frac{4.5.6}{1.2.3}+\frac{3.5}{2.4}\,\frac{6.7.8}{1.2.3}\frac{a^2}{l^2}+\ldots\right)$; we get further

$6P_5 = \frac{a^2}{l^9}\left(\frac{3}{2}\cdot\frac{4.5.6.7.8}{1.2.3.4.5}+\frac{3.5}{2.4}\cdot\frac{6.7.8.9.10}{1.2.3.4.5}\frac{a^2}{l^2}+\ldots\right)$; etc., etc.

On the one hand e.g. $6P_5$ approaches for $l=\infty$ to $\frac{3}{2}\cdot\frac{4.5.6.7.8\,a^2}{1.2.3.4.5\,l^2}$, while on the other hand according to (3) P_5 evidently becomes $=\left(x_5+\frac{13}{2}\right)\frac{a^2}{l^9}$, if x_5 represents the coefficient of l^4a^2. In P_n we have therefore $x_n=\frac{1}{n+1}\frac{3(n+1)(n+2)(n+3)}{2\cdot 6}-\frac{2n+3}{2}=\frac{(n+2)(n+3)}{4}-$

$-\frac{2n+3}{2}=\frac{n(n+1)}{4}$, by which the above is proved.

According to (4) we can now write for $F:e^2$:

$$(z=0)\ \frac{F}{e^2}=2\frac{a^2}{l^5}\left(\frac{3}{2}\cdot\frac{4}{1}+\frac{3.5}{2.4}\cdot\frac{6}{1}\frac{a^2}{l^2}\right)x+2\frac{a^2}{l^7}\left(\frac{3}{2}\cdot\frac{4.5.6}{1.2.3}+\frac{3.5}{2.4}\cdot\frac{6.7.8}{1.2.3}\frac{a^2}{l^2}+\ldots\right)x^3+$$
$$+2\frac{a^2}{l^9}\left(\frac{3}{2}\cdot\frac{4.5.6.7.8}{1.2.3.4.5}+\frac{3.5}{2.4}\cdot\frac{6.7.8.9.10}{1.2.3.4.5}\frac{a^2}{l^2}+\ldots\right)x^5+\text{etc.},$$

or also:

$$(z=0)\ \frac{F}{e^2}=12\frac{a^2}{l^5}x\left[\left\{1+\frac{5}{4}\cdot\frac{6}{4}\frac{a^2}{l^2}+\frac{5.7}{4.6}\cdot\frac{8}{4}\frac{a^4}{l^4}+\frac{5.7.9}{4.6.8}\cdot\frac{10}{4}\frac{a^6}{l^6}+\ldots\right\}+\right.$$
$$+\frac{4.5.6}{2.3.4}\frac{x^2}{l^2}\left\{1+\frac{5}{4}\cdot\frac{6.7.8}{4.5.6}\frac{a^2}{l^2}+\frac{5.7}{4.6}\cdot\frac{8.9.10}{4.5.6}\frac{a^4}{l^4}+\ldots\right\}+$$
$$\left.+\frac{6.7.8}{2.3.4}\frac{x^4}{l^4}\left\{1+\frac{5}{4}\cdot\frac{6.7.8.9.10}{4.5.6.7.8}\frac{a^2}{l^2}+\frac{5.7}{4.6}\cdot\frac{8.9.10.11.12}{4.5.6.7.8}\frac{a^4}{l^4}+\right\}+\text{etc.}\right],$$

i.e.

$$(z=0)\ \frac{F}{e^2}=12\frac{a^2}{l^5}x\left[\left\{1+\frac{15}{8}\frac{a^2}{l^2}+\frac{35}{12}\frac{a^4}{l^4}+\frac{525}{128}\frac{a^6}{l^6}+\ldots\right\}+\right.$$
$$\left.+5\frac{x^2}{l^2}\left\{1+\frac{7}{2}\frac{a^2}{l^2}+\frac{35}{4}\frac{a^4}{l^4}+\ldots\right\}+14\frac{x^4}{l^4}\left\{1+\frac{45}{8}\frac{a^2}{l^2}+\frac{165}{8}\frac{a^4}{l^4}+\ldots\right\}+\text{etc.}\right],$$

for which may be written:

$$(z=0)\quad \frac{F}{e^2}=12\frac{a^2}{l^5}x\left[\varphi_1+5\frac{x^2}{l^2}\varphi_3+14\frac{x^4}{l^4}\varphi_5+\text{etc.}\right] \qquad (4a)$$

Two limiting cases.

1. If l is *large* with respect to a (gases), (4) or (4a) evidently approaches to

$$\frac{F}{e^2} = 12\,\frac{a^3}{l^5}\,x, \qquad (l \text{ large}), \quad \ldots\ldots\ldots \quad (5)$$

and this not only for $z=0$, but also for finite values of z, provided they are not large with respect to l.

This is, therefore, the limiting value of the action, when the moving molecule, with comparatively large distances of the molecules, is between P and Q, if not too close to one of them. The action is in inverse ratio to the **fifth** power of l, and directly proportional to the square of the radius a of the orbit of the electrons. It remains pretty well unchanged at the same value of x, when M moves from O slightly to the left or to the right, but it is on the contrary proportional to the deviation x of the electron in its orbit; hence it is **purely periodical.** For $x=0$ the action is $=0$, but not so for $x=+a$ or $x=-a$, even if M is in the neutral point O.

As regards the action between M and P, resp. Q separately, we have according to (1a) and what was found above:

$$F_1 : e^2 = (P_0 + 2P_1 z + 3P_2 z^2 + \ldots) - (P_0 + 2P_1 (z-x) + 3P_2 (z-x)^2),$$

i.e.

$$\left.\begin{aligned} F_1 : e^2 &= \quad 2P_1 x + 3P_2 (2zx - x^2) + 4P_3 (3z^2 x - 3zx^2 + x^3) + \ldots \\ \text{and likewise} \\ F_2 : e^2 &= -2P_1 x + 3P_2 (2zx - x^2) - 4P_3 (3z^2 x - 3zx^2 + x^3) + \ldots \end{aligned}\right\},$$

so that about half of the total action $(F_1 - F_2) : e^2 = 4P_1 x +$ etc. comes from the atom P; the other part, as the action of a force in the opposite direction, from the atom Q. If e.g. $x=+a$, the electron is as far as possible in the direction of Q, and M will be attracted by P with a force $=$ about $2P_1 ae^2$, and repelled by Q with an almost equal force. (Here $P_1 = 3a^2 : l^5$).

When the gas-molecule M moves towards P, its velocity u will, accordingly, also undergo *periodical* modifications, till it has approached P so closely, that at $l - z =$ about $2a$, the attractive force being still finite at $x=+a$, the repulsive force begins already to approach infinity at $x=-a$. (see § 10); hence the velocity is reduced to 0, after which M moves back (collision).

But with *very small values* of u it may occur that this return already takes place long before P has been reached. We shall revert to this in the next paragraph; it is the well-known case of **gas-degeneration.**

2. If l is *small* with regard to a (*liquids* and *solid bodies*), then $l = 2a$, $z = 0$ may be put, and (4) holds therefore again, in which now, however, P_1, P_2, etc. approach according to (3) to the following values: [1])

$$\left.\begin{aligned} P_1 &= \frac{1}{a^3}\left(\frac{1}{6}\sqrt{3} - \frac{1}{8}\right) = \frac{0{,}1637}{a^3}, \text{ hence } 4P_1 = \frac{0{,}6548}{a^3} \\ P_2 &= \frac{1}{a^5}\left(\frac{227}{1944}\sqrt{3} - \frac{1}{23}\right) = \frac{0{,}1710}{a^5}, \quad \text{,,} \quad 8P_2 = \frac{1{,}3680}{a^5} \\ P_3 &= \frac{1}{a^7}\left(\frac{1103}{11664}\sqrt{3} - \frac{1}{128}\right) = \frac{0{,}1560}{a^7}, \quad \text{,,} \quad 12P_3 = \frac{1{,}8718}{a^7} \end{aligned}\right\},$$

so that now $F : e^2$ will approach

$$\frac{F}{e^2} = \frac{0{,}655}{a^3} x\left(1 + 2{,}089\frac{x^2}{a^2} + 2{,}859\frac{x^4}{a^4} + \ldots\right) \quad . \quad . \quad . \quad (6)$$

It is self-evident that this expansion into series is now only valid for *small values of* x with respect to a. For $x = \pm a$ of course F becomes $= \pm \infty$. I will just point out here, that when in $x = a \sin\varphi$ *means* are taken over all values of φ between 0 and 2π, according to (2) and (4) the total force would become $= 0$ only at $z = 0$. But when z is not $= 0$, hence when M is no longer halfway between P and Q, this is evidently no longer the case (*even* powers of x). And according to the above separate expressions of F_1 and F_2 they do not become $= 0$ at $z = 0$ even when averaged. The separate forces "averaged" with respect to x, and also the mean total force will always be *repulsive* (excepted at $z = 0$ in the last case), because the terms with even powers of x have all of them the – sign. [2]) And this refutes DEBIJE's assertion (see § 8), that without special suppositions (polarisation of the molecules in each other's electric field) the resulting action would always be $= 0$ according to a well-known electric theorem. It is possible to verify by calculation that this is also true for the problem in three-dimensional space.

---

[1]) For the quantities φ_1, φ_2, etc. in (4*a*) the values $\varphi_1 = \frac{32}{12} \times 0{,}6548 = 1{,}7459$; $\varphi_2 = \frac{128}{60} \times 1{,}3680 = 2{,}9184$; $\varphi_3 = \frac{512}{168} \times 1{,}8718 = 5{,}7045$; etc. are easily found.

[2]) This has of course nothing to do with the question of the *Virial* of attraction and repulsion, as in the calculation of this the *time*-average plays a part. Indeed, in the equation of state for gases $\frac{pv}{RT} = 1 + \frac{b}{v} - \frac{a/v}{RT}$ also the Virial of repulsion b/v predominates at high temperatures. This question must afterwards be treated separately.

§ 12. The Equations of Motion. Possibility of Multiple Orbits at Low Temperatures.

Since with small values of z we may write according to (4):

$$F = \gamma_1 x + \gamma_3 x^3 + \gamma_5 x^5 + \ldots \quad (x = a \sin \varphi),$$

this may always be reduced to

$$F = \alpha_1 \sin \varphi + \alpha_3 \sin 3\varphi + \alpha_5 \sin 5\varphi + \ldots),$$

after which integration is possible. But since the expansion into series is not practicable after all for large values of x, in the neighbourhood of $+a$ or $-a$, it is better to retain only the 1st term in the original equation, or to write:

$$F : \gamma_1 x (1 + \lambda_2 x^2 + \lambda_4 x^4 + \ldots),$$

and take the averages with respect to the factor between parentheses. Then we get:

$$F = \gamma_1 x \times \lambda_m = \gamma x,$$

in which γ will be $> 0{,}655\, e^3 : a^3$, when l approaches $2a$. As now $\frac{1}{1/2\,\pi} \int_0^{1/2\pi} \sin^4 \varphi$ is $= \frac{1}{2}$, the same with $\sin^4 \varphi$ will be $= \frac{1.3}{2.4}$, with $\sin^6 \varphi$ is $= \frac{1.3.5}{2.4.6}$, etc., F becomes for $l = 2a$ according to (6):

$$F = \frac{0{,}655\, e^3}{a^3} x (1 + 1{,}04 + 1{,}07 + \ldots),$$

hence averaged many times greater than $\gamma_1 x$. Let us now, for the sake of orientation, integrate the simple equation

$$\frac{d^2 z}{dt^2} = \frac{F}{m} = \frac{\gamma}{m} x, \quad \ldots\ldots\ldots \quad (7)$$

in which γ for small values of x will approach γ_1, when $l = 2a$ (solid bodies and liquids), whereas for large values of l (gases) γ approaches $12 a^3 e^2 : l^5$ (see above).

Thus we find with $\varphi = 2\pi \frac{t}{T} + \theta$:

$$\frac{dz}{dt} = u = u_0 - \frac{T}{2\pi} \frac{\gamma}{m} (a \sin \varphi - a \cos \theta), \quad \ldots\ldots \quad (8)$$

so that duly u becomes $= u_0$ at $t = 0$ (when M passes through the neutral point O). Repeated integration yields for the path passed over:

$$z = \left(u_0 + \frac{T}{2\pi} \frac{\gamma}{m} a \cos \theta \right) t - \left(\frac{T}{2\pi} \right)^2 \frac{\gamma}{m} (a \sin \varphi - a \sin \theta), \quad \ldots \quad (9)$$

which yields $z = 0$ at $t = 0$. It will now further entirely depend on the value of the phase-difference θ (the difference of time between the fictitious passage of the electron through the nucleus, and of M through O), what type of path of periodic movement will be obtained.

1. If $\theta = 0$ (the electron passes (fictitiously) the nucleus from the *right* towards the *left* exactly when M moves in O towards the right in the direction of P), we get:

$$u = u_0 + \frac{T}{2\pi}\frac{\gamma}{m}(a - a\cos\varphi) \quad ; \quad \varphi = 2\pi\frac{t}{T}.$$

The value of u (see fig. 3) will now always be $> u_0$, so that there can only be question of its becoming 0 on collision ($l - z = 2a$).

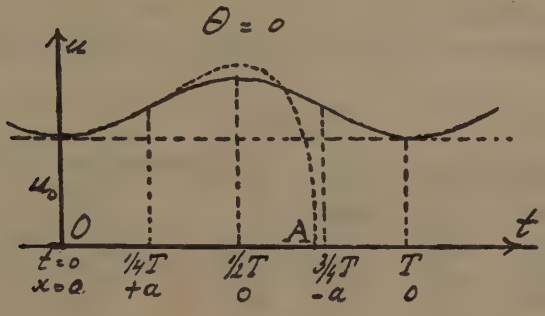

Fig. 3.

The molecule M will then approach P so closely till the electron has assumed the position close to $x = -a$, in consequence of which the repulsive force becomes very great. Then the velocity becomes $= 0$ in an exceedingly short moment, and the molecule is thrown

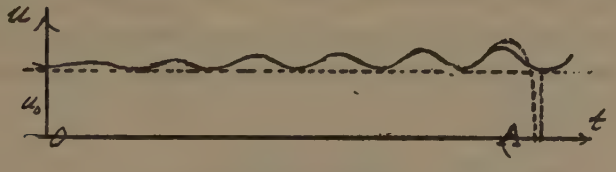

Fig. 3*a*. (Gases).

back (in A, close to $t = {}^3/_4 T$). When the molecules are far enough from each other (gases), several periods may pass before this collision at last sets in (Fig. 3*a*). The increasing values of the amplitudes in Fig. 3*a* must of course be attributed to the increasing influence of z, through which the action exerted becomes stronger and stronger (cf. also the calculations in § 10). This gives also rise to the deviations of the course, following from (8), close to the collision (repre-

sented by the dotted lines). Indeed, for the sake of simplicity we have so far always neglected the influence of z.

We still point out that the magnitude and the sign of the action exercised are always represented in the figures by the inclination of the tangents to the curve.

2. $\theta = 180°$. Then the electron passes the nucleus (fictitiously) just from the *left* to the *right*, when M goes from O to P, and (8) becomes:

$$u = u_0 - \frac{T}{2\pi m}\gamma(a + a\cos\varphi) \quad ; \quad \varphi = 2\pi\frac{t}{T} + \pi.$$

Now the velocity u always remains below u_0 (see Fig. 4). The case of "collision" has been drawn at two successively possible places, viz. at A and A'.

What distinguishes this case from the preceding one, is the possibility that u becomes $= 0$ before the "collision", and the molecule

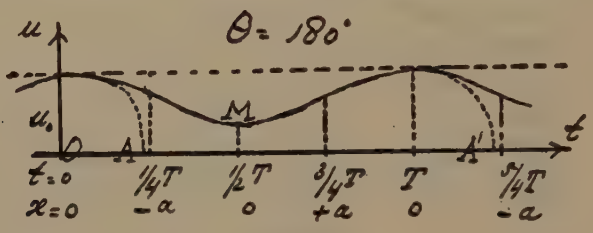

Fig. 4.

accordingly already "*returns*" before P has been reached. This will evidently take place as soon as u_0 is so small that M lies low enough for the curve to intersect the t-axis ($u = 0$) (Fig. 4a). This takes place e.g. in B. Transformed spatially this means that the molecules will move round the position of equilibrium O in **closed orbits**, as soon as we get below the point where the curve touches the t-axis for the first time (**melting point**) [1]). In this case the

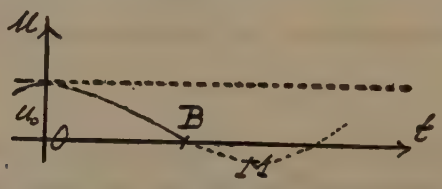

Fig. 4a.

[1]) I may be allowed to anticipate on what follows, and state here that the melting-point calculated in this way for H — if it were realisable — will lie at 36°,4 abs. As this melting-point must lie higher than that of H_2 (because the molecular attraction a that plays a part in it, is greater for H than for H_2), this result is not impossible. (melting-point H_2 lies at 14° abs.).

central force directed towards the centre (O) will always correspond to the resulting repulsion of the surrounding molecules.

Also for *gases* can this take place, but as the distance of the molecules is then greater, hence the oscillations in the value of u much smaller, the limiting value of u_0 (**point of degeneration**) lies much lower than the corresponding value of u_0 (melting-point) for solid bodies. Besides — in accordance with the mutual distance

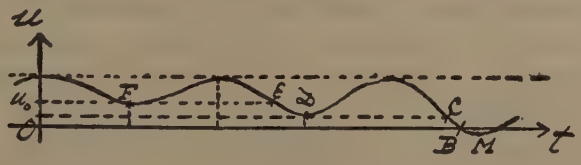

Fig. 4*b*. (Gases).

of the molecules — this transition may take place at different places, during the 1st period, the 2nd, the 3rd period etc. (Cf. Fig. 4*b*, where the return takes place at the third period).

To *given* mutual distance of the molecules (gas density) corresponds, therefore, a definite value of u_0 for which the transition already takes place at the 1st period, (*degeneration point proper*), a value where the transition does not occur until the 2nd period, etc. etc. Here too the molecules will, therefore, revolve round the positions of equilibrium in closed, ever narrower orbits — as the temperature descends.

And thus the phenomenon of **gas-generation** has been explained in a natural way.

When at a given gas-density u_0 becomes too great, then u does not become 0 before the molecule comes in contact with P ("collides"), as is drawn at A in Fig. 3 and 4. Hence no longer any closed orbits (solid bodies above the melting-point; gases above the degeneration point).

It is very remarkable in this, that when u_0 (for gases) becomes gradually smaller and smaller (hence the temperature lower and lower), the place where u becomes zero suddenly *skips* from C to D (see Fig. 4*b*), from E to F, etc. — which corresponds to this that the corresponding *wider* orbit round the position of equilibrium abruptly, hence *discontinuously*, changes into a *narrower* orbit. The latter varies only between D and E, lying *close to each other* (the figure represents time-abscissae, but distance-abscissae of course correspond with them in a corresponding z—u diagram), after which it suddenly skips again to the *still narrower* orbit, corresponding to F. This is then the final orbit, which as u_0 gets still lower, again gradually shrinks. It does not diminish to 0, however, but to a

limiting orbit, which will be discussed later (in connection with the *zero-point energy*) [1]).

And thus an *analogue* has been obtained of the possible quantizised orbits which a negative electron can describe round the positive nucleus. The points D and E lie in this latter case *exceedingly close* together, so that the discontinuity in the value of the radii of the possible orbits is almost complete.

But for this the assumption is required that also for electron and nucleus the force acts periodically, e. g. through this that the positive nucleus executes a *pulsating* movement (analogous to the motion studied by BJERKNES) [2]). It may also be assumed that the nucleus always sucks in "ether" from its surroundings (which is led off to the 4th dimension), the electron expelling ether in the same way. When a rotation is assumed to take place of the electron round an axis coinciding with the direction of the motion, the known equations can be derived of the electro-magnetic field [3]).

But this cannot yet be fully discussed here. One thing at least is certain, that *if* the electrons revolve round the nucleus in *definite* orbits (in which the quantity h plays a part in the determination of radius and velocity), that then necessarily, in consequence of our above considerations, this same quantity h must play a part in the movement of the molecules in closed orbits round positions of equilibrium — in consequence of which that quantity will naturally occur in the relation between E and T which we derived in our previous paper, as analogue of PLANCK's relation; while the quantity ν will be in connection with the time of revolution of the molecules in their closed orbits, which in its turn will again be in relation with the time of revolution T of the electrons round the nucleus — as we saw above.

Clarens, summer 1921. *To be continued.*

[1]) On decrease of temperature such an abrupt succession of some ever narrower orbits is perhaps also possible for *solid* bodies, and this may possibly be brought in connection with some *allotropic* states, which are met with in many elements and compounds.

[2]) Very suggestive in this respect is an old Paper, almost entirely forgotten, by VOIGT in the "Journ. f. reine v. angew. Mathematik", Band 89, on "Der leuchtende Punkt." VOIGT chiefly calculated the state of vibration close to this point, when either a periodic translatory movement, or a periodic rotatory movement was supposed. Later on KIRCHHOFF (Ibid 90, p. 34) considerably simplified VOIGT's derivation.

[3]) The assumption of expulsion of ether from the electron with *the velocity of light* would then also explain that the velocity of the electron can never exceed the velocity of light, and an idea can be obtained of the mechanics of relativity (factor $1-v^2/c^2$).

Palaeontology. — *"On the Cranial Form of Homo Neandertalensis and of Pithecanthropus Erectus, Determined by Mechanical Factors"*. By Prof. Eug. Dubois.

(Communicated at the meeting of Nov. 26, 1921).

All well known fossil men can be ranged around two principal types, differing so much that they must at least be distinguished as *species* of one genus. One type is that of *Homo sapiens*, the modern species of Man. It is true that different races may be recognized also among the fossil representatives of this species. Thus — to mention only the earliest — the eskimo-like race of Chancelade, living in France with a steppe fauna, after the fourth or last Glacial epoch of the Alps (during the dry period, when the trees grew in our country the remains of which we find as the "sand-stubs" under the peat moors); the race of Cro-Magnon, living in France and elsewhere in West Europe during the Reindeer period, that fourth Glacial-epoch (in which the deposition of the upper "Sand-diluvium" took place in the southern part of our country), which race is not to be distinguished from some recent West European and North African tribes. Of somewhat earlier date is the negroid race of Grimaldi, near Mentone on the Mediterranean, almost certainly closely akin to the present-day Bushmen of South-Africa. Probably still older is the australoid Man of Wadjak, Java.

This fact of the differentiation of fossil Homo sapiens into still existing forms, which none of them were at a really lower stage of morphological evolution than the corresponding recent races, leads us to assume for the origin of this type a much earlier date than the time of its earliest remains. This follows immediately after the period of the Mammoth, the third Glacial epoch of the Alps (when the northern half of our country lay covered under an ice sheet), in which time *Homo neandertalensis*, the other species of Man, lived. The Heidelberg Man, a neandertaloid form, which is still more sharply distinguished from the sapiens-type than the Neandertalian proper, which latter died out soon after the Mammoth epoch, lived in the second Glacial epoch of the Alps (during which the upper part of the "Fluviatile Gravel-diluvium" was deposited in the soil of the Netherlands) or — which is more probable, judging from the accompanying fauna — in the second Interglacial or Hippopotamus-epoch (the period of our "potclay"),

Hence the two types of man have undoubtedly existed side by side already very early. This is one of the reasons why we cannot consider the Neandertal Man as the ancestor of Modern Man. A more imperative reason is, however, the very particular specialisation of the Neandertal Man.

The discoveries in France and particularly Marcellin Boule's masterly descriptions have furnished us with very full information about this species. We now know that the Neandertal Man was short of stature, shorter than the smallest recent races, but thick-set, strongly built, and very muscular. He had comparatively short legs and a large head, an exceedingly large face, and walked shuffling, with somewhat bent knees, more on the outer borders of the feet than modern Man; the great toe was so mobile that objects could be picked up from the ground even better than by many naturally living men of the present day. As appears from the absence of the neck-curvature of the spinal column (the presence of which is so characteristic of the species Homo sapiens), the usual attitude of the head was bent forward. The skull is distinguished from that of modern men by numerous morphological characters. It looks like flattened from above, extended lengthwise and breadthwise, with receding forehead, and flat chignon-shaped projecting occiput. The orbital arches have become enormous prominent rounded ridges, the left and the right forming together a torus supraorbitalis, as it is found in most Monkeys and in Pithecanthropus. The mastoid processes are very small, from which it appears that the sterno-cleido-mastoid muscles were rather weak as rotators of the head; they could, however, draw the head vigorously back, assisting the dorsal muscles of the neck, particularly through their further attachment. High reliefs and deep depressions of the nuchal plane of the occipital bone show the exceedingly powerful development of those nuchal muscles, which was required to carry the head, which as a rule was hanging heavily forward; for the face was long, and projects almost like a snout, and the jaws are large. The zygomatic arches stick far out, a feature which is accompanied with masticatory muscles which are directed inward, and are more adapted for grinding mastication. The orbits are very large, the consequently large eyes must have been fit for the formation of large images, as in arborial animals, which must see things accurately close by, and in animals, which need a wide field of view in steppes or deserts. The Neandertal Man found his chiefly vegetable food probably on or in the ground; all investigators have indeed inferred from the premature wear of his teeth that his food was greatly contaminated with earth;

his teeth have very large pulp cavities, were consequently amply provided with blood-vessels and nerves, and therefore very sensitive organs of touch (which are more necessary with vegetarian food than with a carnivorous way of living). The robust lower jaw possesses no chin, or only a rudimentary one; it is strengthened at the place on the inner side (in accordance with the inwardly directed masticatory muscles); the angular part, which is blunted on the edge, is thin and directed inward, like the masticatory muscles, again indicative that the food was rather ground fine than bitten. Nor is the upper dental arcade considerably broader than the lower one, as in Homo sapiens, but the two arcades cover each other more or less. It is remarkable that the form of the nasal aperture differs still more from the simian type than that of Homo sapiens.

It will appear sufficiently from this short description that Homo neandertalensis was specialized to such an extent morphologically and biologically that first, he must be distinguished as a separate species from modern Man — the differences are indeed greater than those by which it is usual to distinguish two species of mammals of one genus —, secondly that the present type of man cannot possibly be derived from him. In many respects modern Man is more primitive, less specialized than the Neandertal Man, just as the African Elephant of these times is more primitive and less specialized than the diluvial Elephas primigenius, the Mammoth.

It is true that so many primitive or pithecoid characters are still very generally ascribed to Homo neandertalensis that this type is put at a lower stage of human evolution than modern Man; but when we have got to know them better, not a few of these characters will probably be explained as the direct consequence of particular physiological, mechanical adaptations, as phenomena of convergence. It is not astonishing that one species of Homo possesses some primitive or pithecoid morphological characters which another lacks, and these strike us particularly in Homo neandertalensis, because we know those of the other, our own species, better. We are, therefore, easily inclined, and think ourselves justified in considering some characters as primitive, which in reality are not so, and in even supposing other primitive characteristics when the species is concerned that is only known in a fossil, i.e. incomplete state. We may expect that the number of these will be greatly reduced on increase of our knowledge, because it has actually already appeared that we have made a mistake in Homo neandertalensis as regards the most important character that distinguishes Man from the Apes, the very great relative brain quantity. Up to

twelve years ago, when only the upper part of the cranium, the calvaria, was accurately known, its shape, which really looks pithecoid, and corresponding small capacity led to the erroneous conclusion that the capacity of the whole cranium should be estimated as very low. Remarkable enough it was assumed in this estimation that the lacking, or at least less closely known, lower part was built after the common human type, in contrast with the pithecoid upper part. Thus as late as 1901 SCHWALBE arrived at a much too low estimation of the total cranial capacity, the volume of the brain, which would have come to no more than 1230 cm^3, although as early as 1898 it had been shown by me, that in Monkeys the upper part of the cranium constitutes a comparatively smaller, the lower cranium a comparatively greater part of the total cranial capacity than in modern Man. The capacity of the lower part of the cranium up to the plane through the frontal cerebral pole and the transverse boundary lines between the cerebrum and the cerebellum is in modern Man about 40% of the capacity of the upper part of the cranium, in most Apes about 60%, and in the very platycephalic large gibbon species, the Siamang, the two parts are even equal. And it is now a priori probable that to the flattened pithecoid upper cranial part of the Neandertal-Man belongs a comparatively large lower cranial part, as in the Apes.

The year 1909 brought a total change in our view of the Neandertal Man, when on direct determination of the cranial capacity of the La Chapelle neandertalian by BOULE, VERNEAU, and RIVET 1530 cm^3 true capacity (1626 Broca) was found. The capacity of other neandertalian skulls can be calculated from the relative length, breadth, and height. As considerable, or not much smaller amounts are found, taking the comparatively small size of the body into consideration, cranial capacities which certainly are not inferior to those of Europeans of this time, and even exceed them.

In 1914 also SCHWALBE was impressed by the evidence that the Neandertal Man is distinguished from the modern human type by the comparatively much greater height of the lower cranial part, which he bounded by the glabella-inion plane. Thus it becomes comprehensible that the total capacity of the cranium can be great in spite of the flattened pithecoid upper cranium, which is little voluminous in comparison with the modern human type. Part of the brain volume has simply moved downward with regard to the said plane, so far as the inion has not been displaced upwards.

It would very well be possible that, notwithstanding the large quantity, the quality of the brain was inferior to that of modern

Man. BOULE and ANTHONY have actually thought they saw this on the endocranial cast; but the cerebral convolutions are imprinted too little distinctly through the dura mater membrane, which was probably particularly thick, and besides the imprint is also incompletely preserved, so that even essential features remain uncertain in the configuration of the surface of the brain. Hence great value cannot be assigned to these researches. Nor is it probable that the proportion between the more highly and the lower organized parts of the brain would considerably differ from that of modern Man.

At any rate the very large brain quantity of the Neandertal Man *in comparison with the Apes* remains a significant fact. Like modern Man this diluvial species possessed more than *three* times the brain volume of anthropoid Apes of equal size (weight). If the shape of the Neandertal Man's skull were a really primitive character, its capacity could not differ from that of an ape's skull to that extent. This leads to the supposition that the pithecoid shape of the skull of the Neandertal Man is no primitive, inherited character, but that it was acquired by convergent development, an adaptation to definite modes of living and due to similar, mechanical causes.

If, therefore, there is no ground to consider the flattening of the upper part of the cranium of Homo neandertalensis as a consequence of low, pithecoid brain-organization, we are naturally led to consider what mechanical factors in Anthropoid Apes can be the cause of, or at least can have influence on the flattening and the formation of the supra-orbital bony ridge, which we also know as the most striking characters of the Neandertal Man.

We then meet with two remarkable facts. First that the skull of the Siamang, the only large gibbon species, is distinguished from all the small species by an equally great flattening and relative decrease of capacity of the upper cranical part together with relative increase of capacity of the lower cranial part [1]), as the Neandertal Man from modern Man (Fig. 1 and Fig. 2 of bisected skulls). In this respect the Siamang may be called the Neandertalian among the Gibbons. This large gibbon species is, besides, distinguished from the small species in that the maxillar and actually the whole facial part of the skull, both as regards breadth and length, is disproportionately much larger and heavier. Consequently the nuchal muscles must carry the head, which hangs forward, with the greater strength, and in conjunction with another muscle apparatus which will be discussed later, and an air-brake

[1]) This applies both with regard to the internal boundary parts and that of the inion, because this occupies the same place in them.

apparatus much more developed in the Orang utan, break the shocks which, arising in locomotion, might injure the skull and its contents.

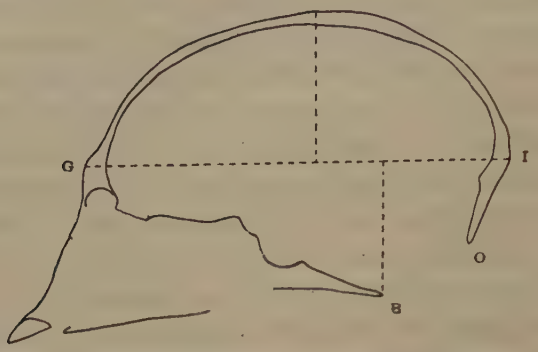

Fig. 1. Median cross-section of the skull of Hylobates agilis. $^2/_3$ of nat. size.

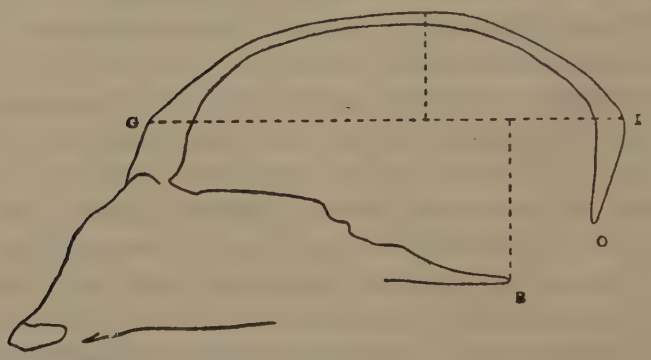

Fig. 2. Median cross-section of the skull of Hylobates (Symphalangus) syndactylus. $^2/_3$ of nat. size.

In connection with these features the foramen magnum in the Siamang lies somewhat more backwards, and its plane stands up somewhat more steeply; also the planum nuchale of the occipital bone is steeper, and the bending of the cranial basis, the basi-cranial axis, is less pronounced than in the small gibbon species.

These are characters of the Siamang which in the comparison of Apes with Man are usually considered as primitive and indicating a lower stage of brain organization, which however, in the comparison with the small gibbon species can decidedly not have such a meaning, but must have geometrical and mechanical causes.

In the calculation of the mean endocranial surface dimensions (the two-third powers of the cranial capacities), and KEITH's palatal areas, it is found that the maxilla of the Siamang is relatively one and a half times as large as that of the small gibbon species, which is undoubtedly in connection with an important difference in the

way of feeding, for the influence on this ratio of the difference in body weight (as 4 : 3), can be but slight. When the two median-sagittal cross-sections of the skull are divided by straight lines *GB* into an upper (cerebral) and a lower (facial) part, it is found, on determination of the area of these parts that per unit of area of cerebral part Hylobates syndactylus has exactly *twice* as much facial part as Hylobates agilis. The ratio between the areas of the cerebral and the facial parts of the skull, which is thus changed, must certainly cause the former to extend more in length than in height, directly by geometric adaptation, but at the same time it must modify indirectly the form of cranium as it imposes higher demands on the muscles that support the head.

The second remarkable fact met with when the Anthropoid Apes are compared, is the great difference of the shape of the skull between the Orang utan and the two other large Anthropoids, the Chimpanzee and the Gorilla. The skull of the Orang utan is vaulted more highly, comparatively short and round, with high forehead and round occiput, and without prominent supra-orbital ridges. Now it is also found that in both sexes of the Orang utan the heavy head is supported in front by a huge air throat pouch, or laryngeal sac (Fig. 3), which (like the small laryngeal sacs found in many other Monkeys) can be filled and emptied at will from the larynx, and serve as buffer or shock reducer and air-brake. The Gorilla, the Chimpanzee, and the Siamang — again in both sexes — have only comparatively small laryngeal air sacs; they are entirely absent in the small Gibbon species. The superior parts of the laryngeal sac of the Gorilla are no more than small pouches stretched between the hyoid bone and the larynx towards the right and the left (Fig. 4), while a median continuation, passing before the windpipe downwards, forms the channel to the subclavian and axillary pouches as in the Chimpanzee. The Chimpanzee has no pouches worth mentioning above, whereas the air pouch of the Siamang is confined to the space between the body of the mandible and the lower side of the larynx, and a continuation towards the chest is wanting.

The significance of the very large laryngeal sac of the Orang utan cannot be ascribed to strengthening of the sound, for its voice is much seldomer heard and is much less loud than that of its African cousins, which even scare away elephants out of their neighbourhood by their terrible roar. Besides the small gibbon species have, without throat pouches, as loud voices as the Siamang.

The opinion pronounced for the first time by Deniker and Boulart, that the laryngeal sac of the Orang utan has the significance of an

air-cushion carrying the heavy head, has been accepted by many zoologists. In fact it is very plausible that the inert Orang utan,

Fig. 3. Laryngeal sac and cheek-lobes of a large male orangutan (reproduced from DENIKER and BOULART). The rather stiff cheek-lobes can prop the head sideways on the air-cushion. Slightly more than 1/5 of nat. size.

which is much less muscular than the African large Anthropoids, which is particularly provided with weak nuchal muscles according to the two mentioned, French anatomists and which as it were, drops its heavy face on his chest, possesses in the laryngeal sac an apparatus to break as by an air brake the shocks to which his cranium and its contents, which hangs more heavily forward than in the other Anthropoids, but which is less efficiently supported by the neck muscles, would be exposed in locomotion [1]).

In the other Anthropoids, which possess a throat pouch, this mechanism is in itself inadequate, so that greater demands are imposed on the cervical muscles with the nuchal ligament to carry the heavy head. But even these, though besides assisted by the air-cushions of the (small) throat-pouches, would be inadequate to protect the cranium and its contents entirely against dangerous shocks without the other above-mentioned muscle apparatus. They are supported in this task by the apparatus of the occipito-frontalis muscle, the musculus epicranius, which is present in all Apes and Man, in

[1]) The inion is placed very high, so that the nuchal muscles (and ligament) support the head only when it is lifted up by the laryngeal sac.

different degrees of development. This muscle, which consists of the flat right and left frontal muscles and the right and left occipital

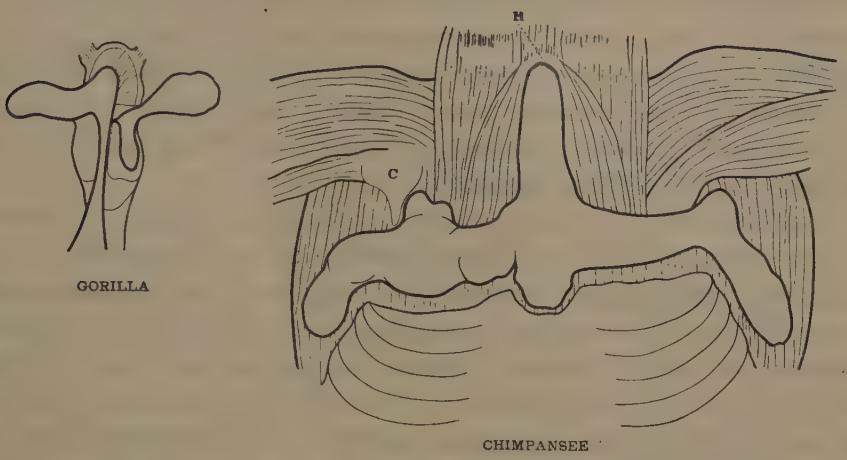

Fig. 4. Upper part of the laryngeal sac of a female gorilla. Laryngeal sac of a female chimpanzee; in this genus the upper part is not developed. (Both reproductions taken from EHLERS). The median air-cushion and the subclavian and axillary cushions protect windpipe and bloodvessels, and also to a certain extent the contents of the cranium against concussions and shocks caused by the heavy head which is thrown forward. *H* hyoid bone, which is placed on a line with that of the Gorilla, *C* coracoid process. 1/4 of nat. size.

muscles with the epicranial aponeurosis, the galea aponeurotica, as intermediate tendon, is stretched out over the upper surface of the cranium. The epicranial aponeurosis is attached loosely and movably with the skull bone, but firmly bound to the hairy skin of the head, which is thus pressed firmly, but elastically against the bone, in every position of the head, by the whole apparatus, forming a mechanical whole with it, and playing a similar part as paper stuck to glass or the iron rods in reinforced concrete, i.e increasing the shearing and tensile strength of the cranium, and at the same time deadening the shocks that might injure the contents of the cranium. The action of the frontal muscle in mimicry can certainly not be the only function, nor the principal significance of this muscle apparatus. This follows already from the fact that the frontal muscle in Man, where it arises for the greater part at the skin under the brows, and is generally only little slightly attached to the bone, draws up the brow and *wrinkles the forehead* transversally, expressing in this way attention and astonishment, whereas in monkeys, where it is more attached to the bone of the supra-orbital ridge, *it smoothens the forehead.* In Man just as in the Monkeys, the occipital muscles are

attached to the bone, the external part of the superior curved lines, above the insertion of the neck muscles. Even if this muscle did not exist and the epicranial aponeurosis were at that place directly attached to the bone, this aponeurosis might serve as "punctum fixum" in the mimic contraction of the frontal muscle of Man.

Accordingly the occipital muscle must certainly also serve another purpose; it can hardly be another than the constant application and rendering elastic of the cranial skin cover.

That actually the skull and its contents need protection against the shocks caused by locomotion, is self evident, and appears clearly from the mechanical arrangement of its connection with the trunk. In most Mammals the head is attached to the end of a neck which deviates but little from the horizontal direction, to which it hangs as at an elastic rod sustained chiefly by the elastic ligamentum nuchae. The foramen magnum and the condyles lie on the back side of the skull and the axis of the spine coincides about with the axis of the brain. In the Anthropoid Apes the brain has become much larger; the foramen magnum has been displaced to the basis of the skull, the vertebral column is more oblique with regard to the brain axis, and with a weaker ligament much stronger neck muscles must sustain the skull and secure it and its contents by their strain against concussions. In modern Man the brain has become very voluminous absolutely and in proportion to the face, the foramen magnum and the condyles are brought forwards to near the middle of the base of the skull, with which base the vertebral column forms a right angle, and the skull is balanced in unstable equilibrium on the spinal column. The latter has a double S-shaped curvature, with convex cervical- and lumbar curvature towards the front, and concave dorsal part, through which it possesses a high degree of elasticity and prevents shocks to the contents of the chest and the skull like the spring of a carriage. Notably the neck-curvature, which is characteristic only of the modern human species, makes that part of the spinal column an elastic stem of the head. *The Neandertal Man did not possess this mechanism, as appears from the form of his cervical vertebrae.* Accordingly the head hung forward, somewhat as in Anthropoid Apes. As, besides, the facial part was heavier than that of modern men, and as in this hanging over also the brain, which had greatly increased in weight in comparison with the Anthropoid Apes, had to be upheld by muscular action, very great demands were imposed on the nuchal muscles (and ligament), also to secure the contents of the cranium against shocks, in spite of the supporting transverse axis of the skull, which

lies much more to the front than in Apes. The insertion of these muscles and bands really reached higher at the occiput, with regard to the internal parts of the skull, than in the races of modern Man with small jaws. But this had to be attended with a more powerful epicranial muscular apparatus, and the shape of the upper part of the skull, although not entirely homologous, came to resemble that of the Anthropoid Apes; the skull had namely to be provided with strong supra-orbital ridges, for the attachment of the strong frontal part of the epicranial muscle.

This, apparently, must be the significance of the supraorbital ridges of Neandertal Man. That this torus supra-orbitalis, which is more pronounced in men, would have enhanced masculine charm in the eyes of female neandertalians, or would have contributed to give a fierce and awe-inspiring appearance in fight, or might have served as a protection of the eyes against the glaring light of the steppes, in which the Neandertal Man lived — these are all interpretations that have found little response. We have certainly to think of more mechanical causes for the origin of these strong bony ridges. Many anatomists consider the reinforcement of this part of the frontal bone as being in connection with the masticatory activity of the strong jaws, an interpretation which certainly deserves serious consideration. But the Orang utan has equally strong or stronger jaws than the Chimpanzee, and in contrast with the latter, it has no torus supraorbitalis. Certainly we have also to think of the circumstance that in the Apes the superior borders of the orbits are more prominent than in modern Man, because the orbits (with their contents, the eye-balls) had to advance under the relatively much smaller brain; the eye-balls of the large Man-like Apes have indeed about the same size as those of Man, the volume of the brain being less than a third of that in Man. In the Orang utan, on account of the existence of the large throat pouch, the brain is greatly displaced frontwards, above the orbits, and the lateral as well as the superior borders, with the roofs and the outer walls of the orbits are not or very little prominent with regard to the forehead. The Neandertal Man, however, possesses enormous supraorbital ridges, but the outer walls of the orbits are not more projecting anteriously than in the modern species of Man [1]). And in the Orang utan we find the epicranial muscle apparatus to be weak, consisting of long thin muscle fascicles with narrow galea, which can partly be brought in relation with the absence of strong bony ridges at the place of the orbital arches. It has, indeed, been shown

[1]) This passage on the orbits in the Apes and Man was by mistake omitted in the "Verslag" of this communication.

by AICHEL how muscular strain, acting on the periosteum, causes growth of bony substance, and how pressure counteracts growth of bone. The formation of the torus supraorbitalis in Neandertal Man can then be explained by the strain of a particularly strong epicranial muscle. Strong pressure from above on the cranial vault by this muscle apparatus must, according to AICHEL's view, inhibit the growth on the upper surface (short sagittal suture!) in contrast with the sides of the skull, thus causing flattening from above.

The flattening of the absolutely very large neurocranium can certainly be explained only for a small part by geometric adaptation to the relatively large splanchnocranium. It seems that particularly the mechanical action of the neck muscles and the epicranial apparatus, through strain at the periost, made the cranium extend backwards and frontwards (long squamous suture!) and caused it to be flattened. The result of this was a favourable displacement of the centre of gravity of the head downward.

In this connection the fact is of great importance that in the Neandertal Man, in comparison with modern Man, and in the Siamang, in comparison with the small gibbon species, part of the contents of the cranium has been displaced from above downward. This in an absolute sense, for the external inion at the skull of the Neandertal Man does not lie higher above the internal inion than in many an Australian skull (24 mm. difference — maximum of Neandertalians — in that of La Chapelle —, 23 mm. in the Australian skull offig. 5), and in all the gibbon species the inion externum occupies an equal place.

In the Frisian skulls of the islet of Marken, artificially flattened according to BOLK and BARGE by a peculiar kind of tightly fitting children's caps, this downward displacement of the contents of the skull is also to be found. It is of a smaller amount in proportion to the slighter flattening than (vertically only about a third part of) that in the Neandertal Man and the Siamang (in which, in comparison with modern Man and the small gibbon species the contents of the cranium has been displaced downwards in about the same ratio of height — about a seventh of the total height). As was already mentioned above, the mechanical efficiency results here from the displacement of the centre of gravity of the head downwards, nearer to the supporting line lying behind the perpendicular of the centre of gravity, and also from the more favourable direction of the muscular force which pulls at the frontal bone over the cranial vault.

In the Australian aboriginal race the head is, indeed, poised on the vertebral column as in all the races of Homo sapiens, but in consequence of the great weight of the jaws the state of equili-

brium is more unstable than in most other living races of man, which requires particularly strong nuchal muscles (and ligament). This is very clearly to be seen by the size, the form, and the external moulding of the lower tabular part of the occipital bone. Calculated in proportion to equal cerebral areas (the two-third powers of the cranial capacities), the Australian human race has about one and a half times larger palatal area than the European (Englishman); in this respect it differs, therefore, almost as much from other living races of Man as the Siamang from the other gibbon species. Among recent Men this race has also the smallest relative height of the calvaria (calvarial height index). In the Neandertal Man it lies between 40 (Gibraltar) and 44.3 (Spy II), and in Australians it reaches (according to BERRY and ROBERTSON) a mean value of 53, however a minimum of 44,9. As a rule at least the median part of the anterior border of the frontal bone, not so very seldom the whole border, is thickened to a bony ridge (torus). But in the last respect the Australians in general distinguish themselves from the Europeans in a higher, in the first respect in a less degree than the Siamang from the small gibbon species. Here the mechanism that has the flattening of the skull and the torus supraorbitalis as morphological consequences, must, again, be different. It seems to me that the great thickness of the skull-bones so characteristic of the Australian race, for a great part takes over the task of the epicranial muscle apparatus, the consequence of which is that the flattening and the torus-formation seldom go so far as in the Neandertal Man, whose cranial bones are accordingly less thick than those of recent Australians, and in whom the mechanism that brought about the flattening, was much more powerful. Though the facial part may not have been much heavier than it is in some Australians, the head of the Neandertal Man, which was not poised on the vertebral column, was carried hanging forward, which renders strong nuchal muscles necessary.

Thus the exceedingly thick-boned Piltdown skull (Eoanthropus) may have been high-vaulted with a comparatively small brain volume, and though accompanied by a large masticatory apparatus.

Towards the middle of November a fossil human skull became known which is exceedingly remarkable, also as regards the subject of this communication. It had been discovered in August of this year in the cavern of Broken Hill in Rhodesia (14° 26′ S.L., 28° 37′ E. L.), and is at present in the British Museum (Natural History) in London. Through the kind information and photographs sent, in the first place, by Dr. A. SMITH WOODWARD, Keeper of the Geological Department of this Museum, and by Prof. ELLIOT SMITH and

Sir Arthur Keith, I was already soon enabled to test the validity of the above considerations also by means of this new cranial type. Besides with the greatest courtesy Dr. Smith Woodward gave me an opportunity to study the fossil skull itself in London. I feel prompted to express my great indebtedness to him also here.

Through its flattening and very pronounced torus supra-orbitalis, and also through its large jaw, the Rhodesian skull makes at first sight the impression of a neandertalian. But a closer examination and study shows that the fossil skull from Rhodesia deviates in many respects from the neandertal type, and comes closer to that of Homo sapiens. Accordingly Smith Woodward [1]) considers the fossil African as a new species: *Homo rhodesiensis*, the accurate systematic place of which cannot be defined as yet. He does not think it impossible that this species is the stage of development following after that of the Neandertal Man in the ascending series. Keith [2]), on the other hand, sees in the Rhodesian a more primitive type than the Neandertal Man, which approaches the common *ancestor* of the latter and of Modern Man.

In my opinion the resemblance to the neandertalian cranial type is only superficial, and confined to the flattening and the torus formation, which may be explained by physiological, mechanical causes, combined with some characters in subordinate connection with these features. He belongs to the type of Homo sapiens, and has particularly much in common with the Australian race.

As an introduction to characterize the skull some of the principal dimensions (compare Fig. 5) may first be given. [3]) The glabella-inion line (the form of the broken of inion could be reconstructed with great accuracy), at the same time the maximum cranial length, measures 207 mm. (The maximum length of the La Chapelle skull is 208 mm.). The maximum cranial breadth is 145 mm. (in La Chapelle 156 mm.) The minimum frontal breadth is 104 mm. (as against 109 in La Chapelle). On the other hand the breadth of the torus is 139 mm. (as against 124 in La Chapelle). The basi-bregmatic

[1]) Nature. Vol. 108. N°. 2716. November 17th, 1921, p. 371—372.

[2]) Communicated in a letter.

[3]) I owe the design of the adjoined norma sagittalis (Fig. 5) taken from the fossil skull, to Mr. W. P. Pycraft, M. R. Anthropological Institute. I drew some particulars of the norma lateralis in it according to measures and photos taken from the skull. The other norma sagittalis refers to the Australian skull N°. 792 of the Anatomical Collection of the Sydney University, described by A. St. N. Burkitt and J. I. Hunter. It has been made after the tracing of the median sagittal section of the skull which I was allowed to compare with the fossil Rhodesian skull in London through Dr. Hunter's kindness.

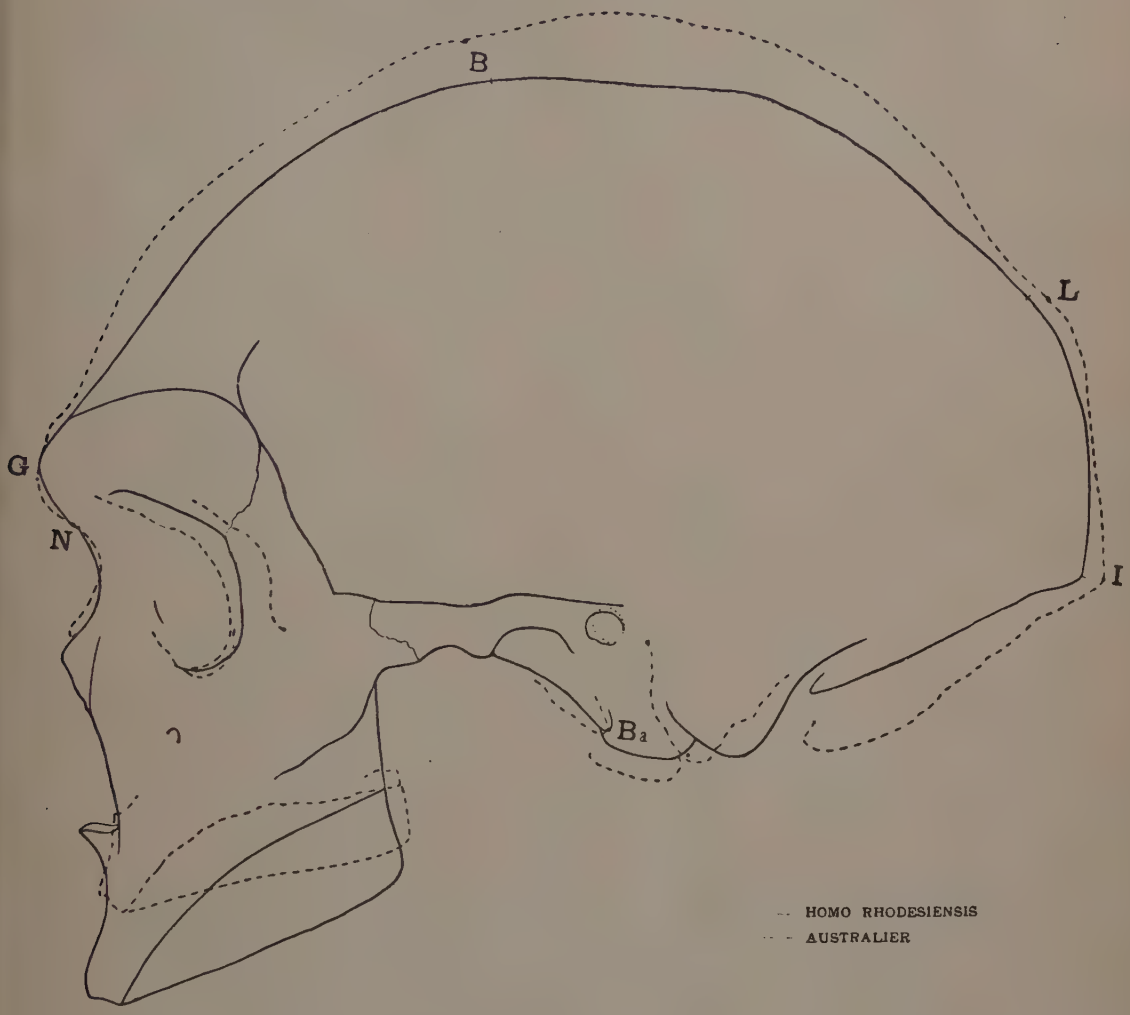

Fig. 5. Norma sagittalis (mediana) and lateralis of Homo rhodesiensis, at $^2/_3$ of nat. size, and Australian N°. 792 Anat. Coll. Sydney Univ., placed at the nasion-basion line, which is made of equal length. The Australian is, accordingly, proportionately somewhat too large.

Some measures (in mm.) of the Australian skull of Fig. 5 are: Glabella-inion line and maximum cranial length 203. Maximum breadth 132. Breadth index 65. Minimum frontal breadth 90. Basion-bregmatic height 133. Nasion-basion line 105. Prosthion-basion line 100. Basion-inion line 99. Nasion-prosthion line 70. Index of calvarial height 48.2. Bregma-glabella-inion angle 51°. Lambda-inion-glabella angle 73°. Opisthion-inion-glabella angle 33°. Nasion-basion-prosthion angle 38°. Cranial capacity 1211 cm^3.

height is 130 mm. (in La Chapelle 131). The basi-nasion line is 111 mm. (as against 125 in La Chappelle).

The length-breadth index is, therefore, 70 (equal to that of Spy I, but smaller than that of La Chapelle, 75). The length-height index is 62,8 (equal to that of La Chapelle). The shorter basal line, the deeper post-orbital constriction, together with a broader face, are important differences with the neandertal-type.

By comparison of the endocranial length (mean of left and right hemisphere 171 mm.), breadth (134mm.), and (internal basion-vertex) height (122 mm.) with the corresponding measures of the La Chapelle skull and of the Australian in the adjoined Fig. 5, I calculate, in proportion to the known cranial capacities of the latter (1530 and 1211 cm^3), taking the comparative slight difference in form into consideration, about 1400 cm^3 in both comparisons for the capacity of the Rhodesian skull, which value, accordingly, will not be far from the truth.

The length of the palate (to the back sides of the third molar teeth) is 59,5 mm., the breadth between the outer sides of the second molars 78 mm. (in Wadjak II 81 mm., in La Ferrassie 71,5). The palate is very high, 19 mm. for *m. 2*. The dental arcade is, therefore, very large (though as with the Wadjak men, the size of the teeth themselves is exceeded by that of many Australians). For the palatal area I find 4000 mm^2, i.e. 140 mm^2 less than for Wadjak II, but 500 mm^2 more than for Wadjak I, 200 mm^2 more than for La Chapelle, 930 mm^2 more than for the La Ferrassie neandertalian.

Striking are the differences between the Rhodesian and the Neandertalian skulls in the following points.

Seen in profile the Rhodesian entirely lacks the snout-like form of the facial skull part, which is so characteristic of the Neandertal Man; in this respect he resembles the Australian. The jaw projects less (Flower's alveolar index, basi-alveolar length $\times$ 100 : basi-nasal length $=$ 105, against 108 in La Chapelle), but it is higher and broader. The nasi-alveolar line is 92 mm., as against 87 in the La Chapelle skull, 70 in our Australian skull. The line is 72.4% in the Rhodesian, 70% in La Chapelle, 51% in our Australian, 48% in other Australians, of the calvarial height above the basi-nasal line. The nasion-basion-prosthion angle is 46° in the Rhodesian skull, as against about 40° in La Chapelle, and 38° in our Australian and in other Australians. The width of the jaw appears from the distance between the outer sides of the second molars. The occiput is formed after an Australian type (very different from that of the Neandertal-skulls), with large, flat planum nuchale, separated from the upper tabular part of the occipital bone, which at that place

projects most backwards, by very considerable superior curved cristae and a distinct external occipital protuberance. The outer wall of the orbit (external angular process of the frontal bone and frontal process of the malar bone) though drawn out thin in temporal direction, is *much* more massive even than in Australian aborigines (minimum orbito-temporal breadth of the frontal process of the malar bone 13 mm.!). This undoubtedly implies that the fascial origin of the anterior part of the temporal muscle, which is chiefly active in biting mastication, was much stronger, hence that part of the muscle itself comparatively more powerful (which is also to be inferred from the deeper post-orbital constriction — smallest frontal witdh). For the rest the whole malar bone is much more robust (the greatest breadth or height under the orbit is 28 mm.), to which a musculus masseter must have corresponded, which was very powerful, particularly in its outer part (again a feature indicating chiefly biting mastication).

In front view the face is broader. The nasal aperture however, is narrower, of the sapiens type; the face measures across the angular processes of the frontal bone (torus-width), as I stated already, 139 mm. against 124 in the La Chapelle skull. The maximum width across the malar bones is 134 (La Chapelle about 125). Very striking is the roof-like form of the frontal part of the cranial vault, instead of the dome-like form of the neandertalians. Nor is the torus supraorbitalis uniformly round and without any special modelling, as in the neandertal type, but more angular, with some division into pars medialis and lateralis by a very considerable supraorbital notch, by the outer side of which the orbital arch in the middle projects towards the orbit, which latter thus gets a more square form, in contrast with the more rounded form in the neandertalian skulls. The whole torus is of an, as it were strongly exaggerated, Australian type. It is more powerful than that of the neandertalian skulls; its greatest height is 22 mm.!

In the basal view the planum nuchale, large and flat, with foramen magnum lying more towards the middle of the base of the skull, is found perfectly the same as in Australian skulls, and likewise separated from the upper tabular part of the occipital bone by very considerable cristae nuchae superiores and a distinct external protuberance. With the basion-nasion length 111 is the basion-prosthion length 117 mm, and the basion-inion line 99 mm. The differences of these anterior basal lines with that posterior basal line are 12 and 18 mm. The differences between the corresponding lines of the La Chapelle skull of 125, 132, and 84 mm. are 41 and 48 mm.

In our Australian these differences are 6 and 1 mm., in two other Australians 6 and 10, 19 and 20, on an average in these three 10 and 10 mm. As the situation of the basion and of the condyles about determine each other, it may be said that in the mechanism of the skull of the Neandertal Man the frontal lever is long and heavily loaded compared with the occipital lever. Also in this respect the Rhodesian distinguishes himself from Home neandertalensis, and resembles Homo sapiens, in particular the Australian race. The direction of the plane of the foramen magnum is also Australian, not neandertaloid. The glenoid fossa and articular eminence for articulation with the lower jaw are of the sapiens type. Very striking is, also seen from below, the greater thickness and more transverse position of the malar bones, whereas the zygomatic arches stick out less laterally. It is noteworthy that the thickness of the wall of the brain-case is less, on the whole, than it is usually found in Australian and even neandertalian skulls.

The relative height of the calvaria (calvarial height index) with regard to the glabella-inion line, is 42.3, as against 40 to 44.3 in the neandertalians, and the minimum 44.9 (according to Berry and Robertson) in Australians. The degree of flattening is, therefore, about the same as that of the Neandertal Man, and is also almost reached by some Australians. In the comparative lengths of the sagittal suture (chord 114 mm.) and the temporal suture (chord 105 mm.) of the parietal bone the Rhodesian is closer to very platycephalic Australians.

In consequence of the receding forehead the inion-glabella-bregmatic angle is only 46°, against 45½° in La Chapelle, and 49° as minimum in Australians (Berry and Robertson). The elevation of the nuchal plane of the occipital bone in the Neandertal Man made the glabella-inion-opisthion angle (lowest inion angle) rise to 44½° in La Chapelle, even 54° in Spy I; it is only 30° in the Rhodesian, agreeing with modern men. It is 33° in the Australian of Fig. 5, and reaches a minimum in this race of 31° (Berry and Robertson). The glabella-inion-lambda angle (upper inion angle) is 72°, in our Australian 73°, the minimum in Australians (Berry and Robertson) is 70°; this angle is from 66° to 69° in the Neandertal Man. In contrast with the latter the lower inion-angle has, therefore, remained small in the Rhodesian, with a small upper inion angle.

Nor do we find the lower part of the cranium of greater depth, with the flattening of the upper part, to the same extent as in the Neandertal Man. In consequence of the flattening of the Rhodesian skull the depth of the lower part of the cranium, below the glabella-

inion line, is greater than in most modern men, viz. 31,6 °/₀ of the total height of the basion to the cranial apex. This is considerably less than in the La Chapelle skull (39,3 °/₀), and not much more than is usually found in Australian skulls, e.g. in the Australian skull of Fig. 5 it is 27,7 °/₀, in the Australoid Wadjak I 28 °/₀.

The mastoid process is, indeed, smaller than in most Australians, in my opinion in consequence of the preponderating significance as nuchal muscle which the sterno-cleido-mastoideus obtains in the mechanism of the flattened skull, but considerably larger than in the neandertalians.

The bones of the skeleton found with the skull present the sapiens type — the tibia because of its slenderness, the articular extremities of the femur in that they lack the bulky character of those of the Neandertal Man.

All things considered I cannot doubt that *Homo rhodesiensis* belongs to the type of Homo sapiens. He exhibits this type decidedly in the most primitive, in the Australian form; he is still somewhat more platycephalic and the torus is even more pronounced than is found in Australians, who already distinguish themselves in these points from the other living races of Man. With greater justice even than the Australoid Wadjak Man and the Talgai-Australian can the Rhodesian lay claim to the name of proto-Australian. The characteristics mentioned are certainly in relation with the exceeding largeness of the masticatory apparatus and the heaviness of the facial part of the head resulting from it. They may be accounted for by similar mechanical causes as in the Siamang in contradistinction with the small gibbon species, the Chimpanzee in contradistinction with the Orang utan, partially also similar as in the Neandertal Man, and thus corroborate the above considerations.

The primitive character of this Australoid human skull appears from the heaviness of the facial part, the splanchnocranium in comparison with the cerebral part, the neurocranium. Calculated to equal area of the brain (the two-third power of the capacity), the Rhodesian has a sixteenth larger palatal area than KEITH's very large-jawed Tasmanian [1]), with 3680 mm.² palatal area and 1350 cm.³ cranial capacity, about a sixth larger than the Australian compared in Fig. 5 (calculated from 3100 mm.² palatal area, derived from the relative length and breadth of the dental arcades), and between a fifth and a fourth larger than Wadjak I. It is very important that here, as in the Wadjak Man and the Neandertal Man (incl. the Heidelberg Man) retrogression of the masticatory apparatus more than increase of the brain indicates the direction in which the genus Homo has developed.

[1]) Few Australians reach 3600 mm.², certainly also few Tasmanians.

Of the numerous fragments of bones of animals found in the same cave with the remains of the Rhodesian Man some have been identified as belonging to species still living in Rhodesia or to others only slightly different from these. This renders it probable that the time of his occupation of the cave even falls after the Plistocene period. It seems that the bones are not petrified in the ordinary way, but they are impregnated with phosphates of lead and zinc. The Rhodesian Man may all the same be an archaic form; in the same way the living Ocapia comes closer to the tertiary Helladotherium and Samotherium than to the Giraffe.

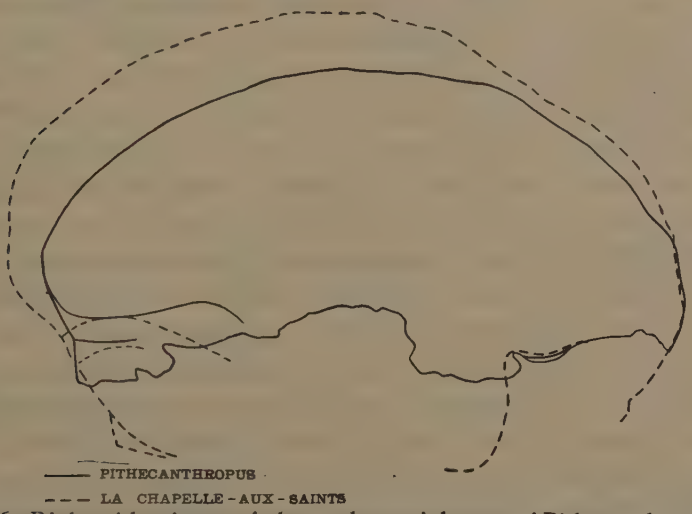

Fig. 6. Right side-views of the endocranial casts of Pithecanthropus erectus and the La Chapelle-aux-Saints neandertalian. Both, especially Pithecanthropus, somewhat more than ½ of nat. size.

The above considerations are also valid for the fossil Ape Man of Java.

The shape of the calvaria of *Pithecanthropus erectus* is almost entirely the same as that of the small gibbon species; only the parietal region is somewhat more flattened. This resemblance can only be seen by the endocranial cast, as the nuchal tabular part of the occipital bone is defective, also as regards its thickness. This nuchal tabula is almost as steep as in gibbon skulls, quite different from that of the Neandertal Man (Fig. 6); the external profile line of the defective calvaria does not show this. Besides — different from what was inferred by many investigators from the plaster cast — the post-orbital constriction has a pithecoid situation, as indeed the proportion between the volume of the orbits and the cranial cavity is not human, and the greatest breadth of the skull lies as far towards the occiput as in the Gibbons; — in this post-orbital region the calvaria is defective on the left. Thus from the capacity of the superior part

of the cranium measured accurately (with water) the capacity of the whole cranium can be calculated with pretty great certainty at about 900 cm^3. by comparison with that of the small gibbon species. That is: with equal body size (weight), which may be estimated by the femur, double the capacity of the large Anthropoids, and more than two thirds of the cranial capacity of Modern Man (male Australian). The platycephaly, even of Pithecanthropus, which is much more considerable than that of the Neandertal Man, cannot be explained sufficiently as a primitive i. e. merely inherited character. With such a cranial capacity the platycephaly would have to be much less pronounced, if it were not adaptational. The form of the cranium of Pithecanthropus determined by about the same mechanical factors as in the small gibbon species, must also have gone together with an almost perfectly gibbon-like facial skull. Consequently the Ape Man carried his head in almost the same overhanging attitude as the small gibbon species, though the femur shows his completely erect posture and gait. It may, therefore, be assumed that the nuchal muscles were exceedingly powerful, and the stress of the epicranial muscle apparatus very great, because in uniform skulls the areas increase only by squares, the weights — hence the muscular forces proportional to them — by third powers. Therefore the very strong flattening of the parietal region and the development of the torus supraorbitalis which is about the same as in the small gibbon species. The rooflike shape (scaphocephaly), which in Pithecanthropus is confined to the frontal bone, as in Homo rhodesiensis, and likewise the scaphocephaly which extends further as far as the parietal bones (lophus of Sergi) of Australians, Tasmanians, Eskimos and others, may be accounted for by the strain exerted as far as the median line on the periosteum by the anterior part of the temporal muscles, which part, active in biting mastication, is then particularly powerful. This strain increases towards above, because the opposed strains of the two muscles meet in the median line. The mechanical efficiency of the roof shape lies in increase of resistance of the cranial vault against the violence of the temporal muscles.

The brain quantity, which is much too great for an Anthropoid of equal bodily size (weight), proves here certainly higher brain-organization. Judging by this relative quantity and the configuration of the cerebral convolutions of the frontal lobe, clearly impressed on the interior surface of the skull-cap, Pithecanthropus presented a closer resemblance to Man than to the Anthropoids. Nevertheless the cranial shape is almost absolutely ape-like; a proof that really even this form was determined partially by *analogous* mechanical factors.

Physics. — *"Double refraction by regular crystals"*. By Prof. H. A. LORENTZ.

(Communicated at the meeting of November 26, 1921).

1. It is well known that crystals of the regular system are anisotropic as to their elastic properties. Their three constants of elasticity are not connected by the same relations as those of isotropic substances. Therefore geometrically equal rods cut from the crystal in different directions are bent or twisted to different degrees.

Substances as rocksalt and fluor-spar on the contrary are single refracting to a first approximation. The FRESNEL ellipsoid from which in crystal optics all phenomena are derived, is a sphere; this is also in accordance with the electromagnetic theory of light on the assumption that the optical properties are defined by the dielectric constant. Crystals with three equivalent mutually perpendicular principal directions can have but one dielectric constant.

More detailed considerations however teach that this optic isotropy can only exist as long as the distance δ of the molecules is very small compared with the wavelength λ. When λ becomes of the same order to this distance, we have for each direction of propagation two mutually perpendicular directions of vibration, the "principal directions" to which belong different velocities of propagation.

2. In 1877 I was led to the treatment of this problem [1]) by the discussion of the explanation of the chromatic dispersion that was often excepted in those days. The unequal velocities of rays of different wavelength were explained by the assumption that the mutual distances of the molecules may not be neglected compared with the wavelength, which assumption may f.i. still be found in old papers of KELVIN. In the cited paper I explained how this assumption is in contradiction with the fact, that with a few exceptions, the regular crystals are single refracting; when namely the ratio δ/λ was so great that it could give rise to the dispersion, this should necessarily be accompanied by a detectable double refraction.

[1]) H. A. LORENTZ: Over het verband tusschen de voortplantingssnelheid van het licht en de dichtheid en samenstelling der middenstoffen. Verh. der Akad. van Wetenschappen te Amsterdam, 1878.

The true value of σ was then taken from the first estimations of VAN DER WAALS. Nowadays however we know the absolute dimensions of the molecules and also the distance σ. Moreover, thanks to the investigations on the interference of the Röntgen rays we can trace the structure of the crystals in detail. This made it desirable to take the problem at hand once more. While the wavelength of the Röntgen rays is comparable with σ, the question is, whether already for light rays the molecular discontinuity is of influence, whether there are any indications that σ/λ may not be quite neglected.

In my former calculations I made use of the theory of MAXWELL in the form given to it by HELMHOLTZ. I followed this way because I had not yet penetrated deeply enough into the ideas of MAXWELL. In the first place, therefore, the calculations had to be repeated and to be based upon the theory of MAXWELL and the theory of electrons. The new calculations gave the same results as the first ones.

3. It will suffice to consider a cubical arrangement of the molecules. The equations for the light motion were derived on the supposition that equal particles are placed at the points of a cubic lattice. Further it has been assumed that in each molecule an electric force is excited and a corresponding electric moment in the direction of that force.

From considerations on the symmetry of the crystal we may easily deduce that for some definite directions of propagation comparable with the axis of monoaxial crystals we have only one velocity of propagation; these directions are those of the edges of the cubic lattice and of the diagonals of the elementary cube. By the diagonals of the side-faces of this cube however those directions of propagation ars given for which we may expect (and this is confirmed by the experiments) the anisotropy in question to be felt most strongly. Further on we shall always assume the direction of propagation to coincide with such a diagonal of a side-face of the cube. Then the principal directions of propagation R_1 and R_2 may be indicated immediately.

The first one is that of the edge of the cube perpendicular to that side-face, the other one that of the second diagonal of that face.

The velocities of propagation belonging to these directions of vibration will be indicated by v_1 and v_2, while the corresponding values of the refraction index $\frac{c}{v_1}$ and $\frac{c}{v_2}$ (c the velocity in vacuum) will be represented by μ_1 and μ_2. For the difference between these last

quantities we find by making use of some simplifications that are allowed because of the smallness of this difference:

$$\mu_1 - \mu_2 = 0{,}11 \frac{n^2 \delta^2 \mu (\mu^2 - 1)^2}{\pi c^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Here n is the number of vibrations in the time 2π, δ the distance of the molecules viz. the edge of the cubic lattice and μ one of the refraction indices μ_1 and μ_2 or rather the mean of both; we may say the observed refraction index.

From (1) we see that the sign of $\mu_1 - \mu_2$ is the same as for a plate of calcite the optical axis of which coincides with the direction R_2 and also the same as for a glass plate that is compressed in the direction R_2.

During the propagation in the crystal over a distance D a difference of phase arises between the vibrations in the directions R_1 and R_2. Expressed in periods or wavelengths this difference in phase is determined by

$$\omega = (\mu_1 - \mu_2) \frac{D}{\lambda} = 0{,}44\, \pi \mu (\mu^2 - 1)^2 \frac{D\delta^2}{\lambda^3}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where λ is the wavelength in vacuum.

For rock-salt $\delta = 2{,}80 \,.\, 10^{-8}$ cm.

With this δ we find for a thickness of 1 cm the following values of ω.

$\lambda = 5{,}9 \,.\, 10^{-5}$ (sodium light);	$5{,}1 \,.\, 10^{-5}$;	$3{,}1 \,.\, 10^{-5}$;	$1{,}8 \,.\, 10^{-5}$
$\omega = 0{,}016$	; 0,025	; 0,14	; 2,1.

Even in the visible spectrum these numbers are great enough to let us expect that under favourable circumstances the effect of the double refraction will be detectable between crossed Nicols.

4. I have sought for the phenomenon in several pieces of rock-salt, in which the faces of the cube were obvious and which were bounded by two side-faces perpendicular to the direction indicated above with L; the distance between these phases was about one cm The side-faces could be easily polished, but we met with the difficulty, that they loose their polish even when the crystal is kept in dry air. To avoid this the crystal was put into a tube a little longer than the tickness of the rock-salt and shut on both sides by glass plates from HILGER with neglectable double refraction. The remaining space in the tube was filled with a mixture of carbonic disulphide and benzol, of which for the mean yellow light, the refraction index is equal to that of the rock-salt. Under these circumstances the crystal is beautifully transparent even when the side-faces have not

been polished particularly well. Neither is it now necessary, that these faces are perfectly plane and exactly parallel to each other; it suffices when the glass-plates are rather exactly parallel to each other. We had only to take care that these plates are not exposed to a too high pressure, so that the closeness (with caoutchouc rings between the glass plates and plane metal rims) was not perfectly tight. This however was of no importance.

5. When after having been fixed in this way the crystal was placed between crossed Nicols the light reappears again and when we fix the eye on the crystal we see irregular light spots over its extension (about 1 cm²). These are due to inner structure deviations and tensions and might retain us from further investigation, when not the differences in phase in this "accidental" double refraction proved to be rather small. When anywhere they reached the value of half a wavelength we should see dark spots between parallel Nicols. There is no question of this; a rotation of one of the Nicols over a few degrees from their crossed situation sufficed to obtain a uniform distribution of the light.

The irregular differences in phase being so small, it might be expected that the effect of a regular anisotropy, the same over the whole cross-section of the light beam could be observed when it was superposed on the irregular differences. In fact this was proved to be the case when a thin plate of mica was adjusted before the crystal and rotated in its own plane.

6. Now the experiment was made on the following way. A small round aperture into which the rays of a glow-lamp with opaque bulb are folling is placed in the focus of a collimator lens. After passing this lens the rays fall in a telescope that is focussed for parallel rays.

We then see a sharp image of the lighted aperture, which image is extincted by two Nicols placed between the collimator lens and the telescope.

When now the crystal is placed between the Nicols and when these are rotated, we see in positions differing 90° minima of the intensity of the light. These minima are not always equally pronounced, but they are always easy to recognize.

That in the mentioned positions the light is not perfectly extinguished, must of course be ascribed to the accidental double refraction, which now however causes a uniform illumination. As we have namely focussed not at the crystal but at the lighted aperture,

we have at every point of the image rays that have crossed the crystal at different points of the section. We may say, that of that double refraction we observe the mean over the whole section.

It may be remarked, that we can also speak of such a mean with respect to the succeeding layers of the crystal and that the great thickness which we use is in a certain respect an advantage. When namely the disturbing double refraction changes irregularly along the ray we may assume the intensity caused by it to be proportional with the thickness, while a regular double refraction gives an intensity proportional with the second power of the thickness as long as the difference in phase is small.

By these disturbing anomalies a sharp focussing at the minimum is impossible. In order to form an estimation of the accuracy of the experiment and therefore of the regularity of the phenomenon we read the positions of the crystal on a divided circle after having focussed at a minimum as accurately as possible. When one of the crystals was rotated continually we obtained the following readings in degrees:

17	107	202	294
14	105	196	293
19	108	196	291

When the numbers of the second column are diminished by 90, those of the third one by 180 and those of the fourth one by 270 and when we then take the mean of the 12 numbers, we find 18, so that the principal positions would be

18	108	198	288

With the other crystals we obtained similar results, sometimes a little better, sometimes a little worse.

8. The positions of extinction were always those, for which the directions indicated with R_1 and R_2 coincide with the directions of vibration of the Nicols. As to this, the theoretical expectation is therefore confirmed.

In order to see whether also the sign of the double refraction agrees with (1) we used a glass plate compressed in one direction which was placed in the way of the rays and which was rotated in its plane. It was found then, that for all ten crystals that were investigated and which were cut from different pieces of rock-salt, the effect could be compensated by the glass plate, when the direction of compression coincided with R_2. Taking into consideration § 4 we come to the conclusion that the sign is opposite to that given by form (1).

Though this contradiction is not satisfactory, our astonishment must not be too great, to my opinion, as in the light of our present knowledge, the theory from which the equation has been derived is so very imperfect. There the assumption has been made that equal molecules were placed at the points of the crystal lattice and that in each of these an electric moment is excited as will be the case when the molecule contains a quasi-elastically and moreover isotropically bound electron. According to the present opinion however the sodium and chlorine nuclei are placed alternately along each edge of the lattice, while round these nuclei and perhaps also round the lines of connection electrons are circulating. When this circulation takes place in planes, the position of these planes may give rise to an anisotropy.

Perhaps the only thing that can make plausible the old theory is, that an anisotropy may be expected which like that determined by (1) is proportional with $\frac{d^2}{\lambda^2}$. I have not tried a calculation based upon the new points of view. First we shall have to be further in the general treatment of light vibrations.

That the old theory is imperfect in several respect may be seen from the following. The difference in phase determined by (2) strongly increases with diminishing wavelength and when working with white light, we should therefore see the field distinctly coloured. This is however not at all the case.

As to the value of the difference in phase, it has hardly been possible to determine it because of the imperfectness of the extinctions. For the crystals used it could not be measured with the compensator of BABINET. The only thing that could be done was to determine with this means the difference in phase of the compressed glass plate by which the double refraction of the rock-salt was compensated rather satisfactorily. In this way it was found, that the difference in phase was a small fraction, about $\frac{1}{20}$ or $\frac{1}{40}$ of a wavelength.

The idea suggests itself to work also with crystals, the side-faces of which are perpendicular to an edge of the lattice, in which case a double refrection as has been described above cannot exist.

To my astonishment even now we can often distinguish two mutually perpendicular positions in which the intensity is a minimum, so that we get the impression that also for the mean of the accidental double refraction taken over the cross-section of the crystal, we can speak of two principal directions. The phenomena however were doubtlessly less regular than for the crystals with which the

former experiments had been made. The minima were less pronounced and with the exception of a single case we could not focus on them as accurately as in the preceding case. Sometimes the focussing was much more dubious. Besides the positions of extinction were not always the same; sometimes the two edges of the lattice which were now lying in a plane perpendicular to the light beam, nearly coincided with the directions of vibration of the nicols, but sometimes they made with these angles of 30° or 40°. My final impression is however, that the double refraction in question really exists; but I hope that others will have the opportunity to repeat the experiments with better crystals than those that were at my disposal.